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REVISION OF WOLF'S SUNSPOT RELATIVE NUMBERS.*

PROFESSOR A. WOLFER.

The next number (XCIII) of *Astronomische Mittheilungen* will contain a new edition of Wolf's table of relative numbers, in which not only will some inaccuracies of the earlier tables—partly errors of computation, partly typographical errors—be expunged but those older observations that have come to light since the tables were compiled, but have not yet been worked up, will be used in the revision. For the most part these new observations were made at Kremsmünster during the years 1802-1830 and have furnished a very valuable addition to the record of sunspots. I now send you a copy of this corrected and completed series (Table I), entitled "Observed Sunspot Relative Numbers." This table, extending from 1749-1901, replaces that published by Wolf in 1880 in No. L of the *Astronomische Mittheilungen*, as well as the various copies which afterwards appeared in the *Meteorologische Zeitschrift* and in the *Memorie della Societá degli Spettroscopisti Italiani*. [It also replaces the table on pages 505-506, *Monthly Weather Review*, November 1901]. It contains no error, and is now to be regarded as definitive so long as the complete new reduction of the whole amount of observational material is not executed, for which the preliminary work is now going on; this will, however, apparently require five or six years more.

Those numbers in the above-mentioned table that are entered in heavy-faced type depend wholly on actual observations; those in light-faced type depend in great part upon actual observations, yet also have in part been obtained by means of graphic interpolations between the days of any month that contained observations with considerable gaps between them: the interpolated

* From the *Monthly Weather Review*, April 1, 1902, edited by Professor Cleveland Abbey, Washington, D. C. Professor Wolfer of France has recently completed a revision of the sunspot relative numbers which replaces all previous publications. Professor Abbey put these numbers in graphic form which we have reproduced in cuts on pages 454 to 457. Tables of these numbers referred to in the above article are omitted because they are too bulky for our space.

numbers were combined with the observed numbers in the computation of the monthly means. Only a very few monthly means, in the eighteenth century, depend entirely upon interpolations; by far the larger number are based upon actual observations. But every monthly mean in which even a single interpolated number has been used is shown by light-faced type; in this respect the distinction may have been too rigorous rather than indulgent, and the light-faced type are, therefore, in no sense to be regarded as an indication of the unreliability of the corresponding numbers.

I have thought that it would perhaps be agreeable to you also to possess new editions of the two other tables that are based upon the preceding, namely, the table of "Smoothed relative numbers" and that of the "Epochs of maxima and minima," which are directly deduced from the preceding. In No. XLII of his *Astronomische Mittheilungen* Wolf has published the smoothed numbers up to 1876, inclusive, and reproductions of these are found in various periodicals and other publications. But there are some errors of computation in this table, and numerous typographical errors occur in the reproductions.

The smoothed relative numbers of Table 2 present the mean course of the spot phenomena; that is to say, without the numerous secondary short periodical variations that really occur in addition to the 11-year variation. Investigations into the general course of the phenomena and the periods of a higher order should therefore, be based upon these smoothed numbers and not on those observed. The method of formation of these numbers has been explained by Wolf, in No. XLII of his *Astronomische Mittheilungen*. The mean of every twelve consecutive observed monthly relative numbers is taken, and every pair of two consecutive means is again united into one mean value according to the following scheme:

$1/12$ (I + II + III.....+ XII) = n_1, for epoch July 1.
$1/12$ (II + III + IV.....+ XII + I) = n_2, for epoch Aug. 1.
$1/2$ ($n_1 + n_2$) = r, which is the smoothed number for mid-July.

This method of smoothing is conformable to that which Wolf has used for eliminating the annual period of the variations of magnetic declination when comparing the latter with the solar spots. This consideration was not necessary for the relative numbers but the combination of twelve months into one mean has been adopted in order to secure a uniform method of treating both phenomena. Table 2, which contains these smoothed

values, has been newly computed from beginning to end and is entirely free from error.

The epochs of maximum and minimum, as deduced from the new Table 2, are given in Table 3; they differ from those published by Wolf (No. LVI of his *Mittheilungen*) only in respect to the maximum of 1805, which, by reason of the newly added observations at Kremsmünster, is changed from 1804.2 to 1805.2. On the other hand, in the later reproductions of Wolf's table there are some typographical errors even in that which Wolf himself published in the year 1890, in the first volume of his Handbook of Astronomy. The present Table 3 is for the moment to be considered as definitive and free from error. I have attributed definite weights to the individual epochs, in order to indicate to those who make use of them some idea as to their reliability. The epochs since 1823, namely, since Schwabe's observations began, all receive the weight 10; for epochs preceding these I have again carefully checked the observational material on which they are based and compared them as to completeness and uniformity, and I believe that the estimate of relative reliability of the individual epochs which I have attained is not far from the truth. With the epochs and weights of Table 3, I have also newly computed the mean length of the 11-year period and the normal epochs of maximum and minimum, and find the values given in Table 3; these differ only very slightly from Professor Newcomb's results*, although the epochs used by him differ partly from the new ones, being older values such as Wolf had published in 1872, but had afterwards changed.

As concerns the 35-year period discovered by Dr. Lockyer, it is quite certain that the interval between each minimum and the following maximum, or the so-called duration of increase, as well as the duration of decrease of the sunspot curve, shows variations in the different periods that are not accidental and that most probably have a periodic character. But I do not believe that the material used by Dr. Lockyer is sufficient to give the length of the greater period with any certainty, because the whole interval of time that Dr. Lockyer takes into consideration covered only one and a half of these greater periods of thirty-five years each. As soon as I received the memoir of Dr. Lockyer I at once investigated whether the existence of this suspected period could be traced backward. This attempt was thoroughly justified, because at least since 1750, the epochs of maximum and

* *Astrophysical Journal*, XIII, No. 1.

minimum are relatively well ascertained and are much more reliable than Dr. Lockyer seems inclined to allow. With the exception of the years 1784 and 1788, which have the weight of 4 ascribed to them in Table 3, there is no other epoch whose uncertainty amounts to one-half year. In Table 4 you will find the result to which I at that time arrived; column *a* contains the interval of time elapsing between a minimum and the following maximum; these are therefore the numbers which, according to Dr. Lockyer, should follow a 35-year period; these are displayed graphically in curve I, fig. 1, and leave no doubt as to their variability; in fact this variability seems to be much stronger than Dr. Lockyer found from the periods used by him. Apparently, they vary to and fro, but a constant period can not be found therein. The great variation between 1780 and 1835 would in fact rather point to the period of fifty-five years, which was previously suspected by Wolf, and would in no case make a 35-year period probable.

Dr. Halm of Edinburg* has also drawn attention to the variability of the interval between minima and maxima, and in fact, this variability is still more plainly brought out by the method of treatment adopted by him, in that he takes account of the ratio of the ascending to the descending part of the 11-year spot period, instead of considering the absolute duration of the ascending portion, as Dr. Lockyer has done. The accompanying Table 4 contains, the interval of time, *b*, between the maximum and the following minimum, as also the ratio *a/b*; the latter is also shown graphically in curve II, fig. 1. The variability of the ratio *a/b* is very striking, and shows in general the same course as in curve I, but there is no indication of a regular recurring period, and especially one of thirty-five years.

Only one result which Dr. Lockyer has brought out is certain, and one which was also demonstrated by Dr. Halm, both from the observed facts and as a consequence of his new theory of the periodicity of the solar phenomena: namely, that the just mentioned variations of *a* and of the ratio *a/b* proceed parallel with the intensity of the development of the spots through each 11-year period in that a maximum follows a minimum quicker in proportion as the maximum is higher, or as Dr. Halm expresses it—"in the individual spot periods the maximum occurs earlier in proportion as the development of the spots is more rapid." Dr. Halm has compared the ratio *a/b* with the absolute altitude of

* *Astronomische Nachrichten*, No. 3723.

the individual maxima, as expressed by Wolf's relative numbers; on the other hand, Dr. Lockyer bases his position on the total sum of the spotted area for each individual period. We attain to the same results if, for comparison, we use the mean annual relative number for a single 11-year period, namely, the sum total of the annual relative numbers for the whole period divided by the length of the period, therefore, $r_{mean} = \frac{\Sigma r}{P}$. The curves III and IV fig. 1, present the values of r_{max}. and r_{mean}. As we see, the agree-

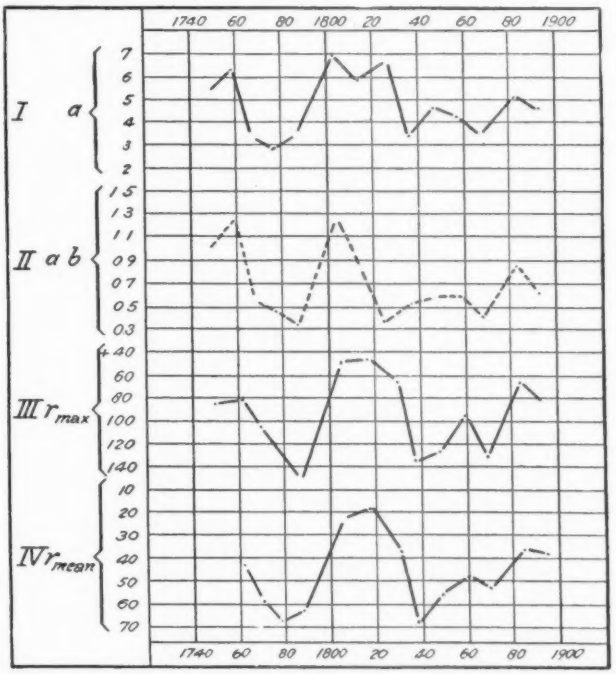
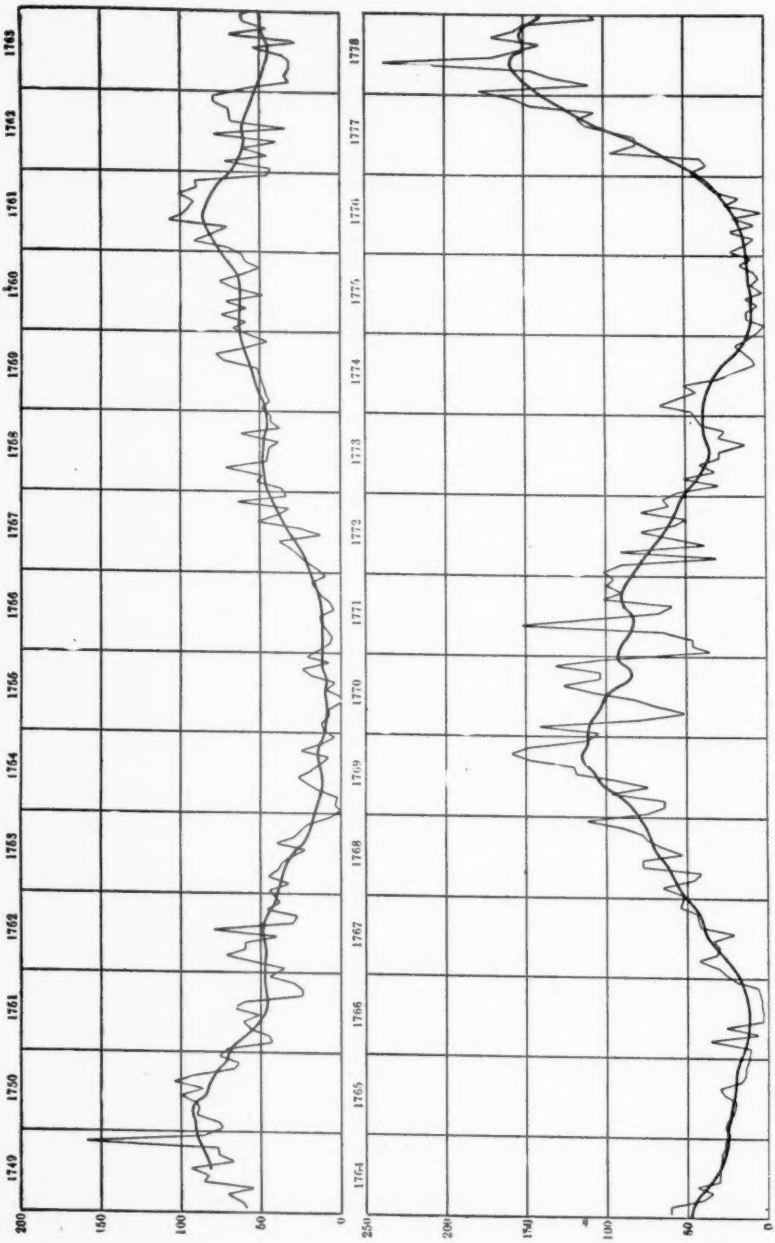


FIGURE 1.

ment with I and II is quite remarkable. That the interval between minima and maxima depends intimately upon the intensity of development of the spots during that period would seem therefore to be quite certain, according to Lockyer's and Halm's investigations; both these quantities are subject to variations of an apparently periodic character; but it seems to me that the material actually available does not indicate a somewhat regular periodicity; and that the continued existence of a 35-year period is not yet demonstrated.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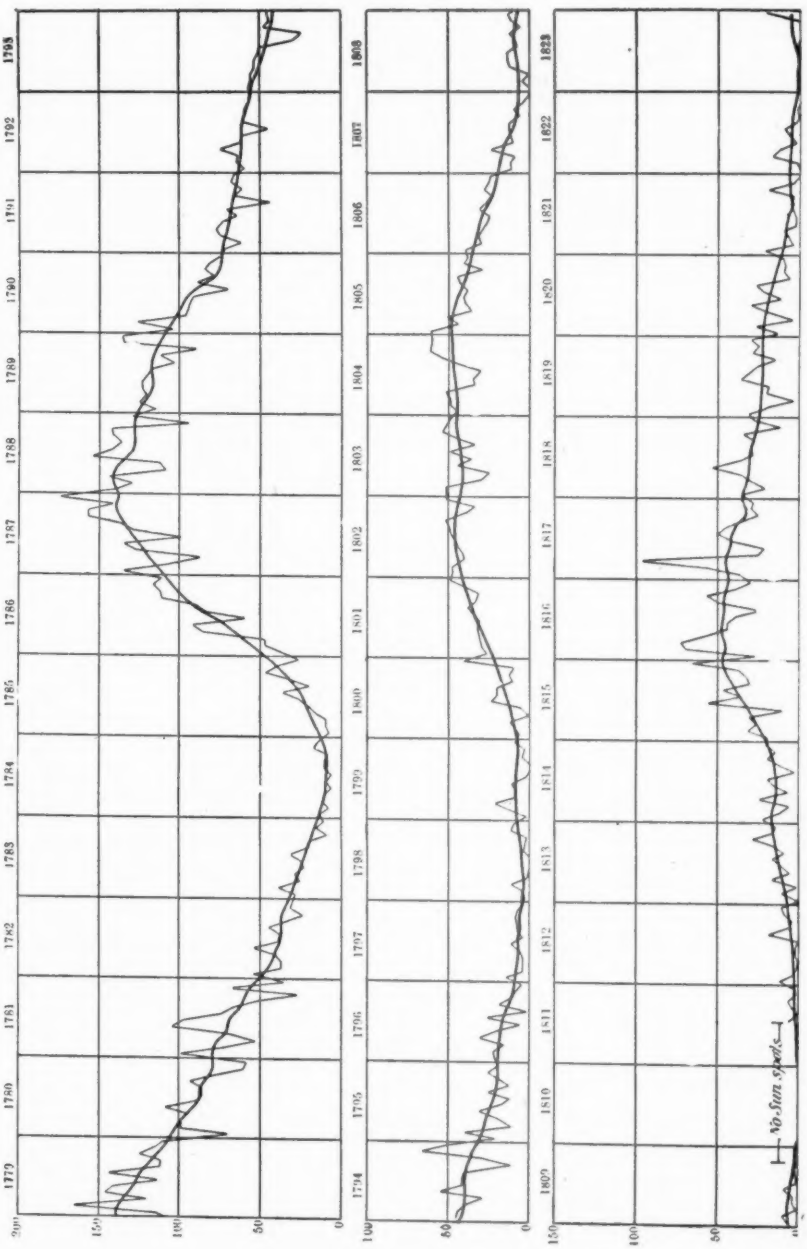
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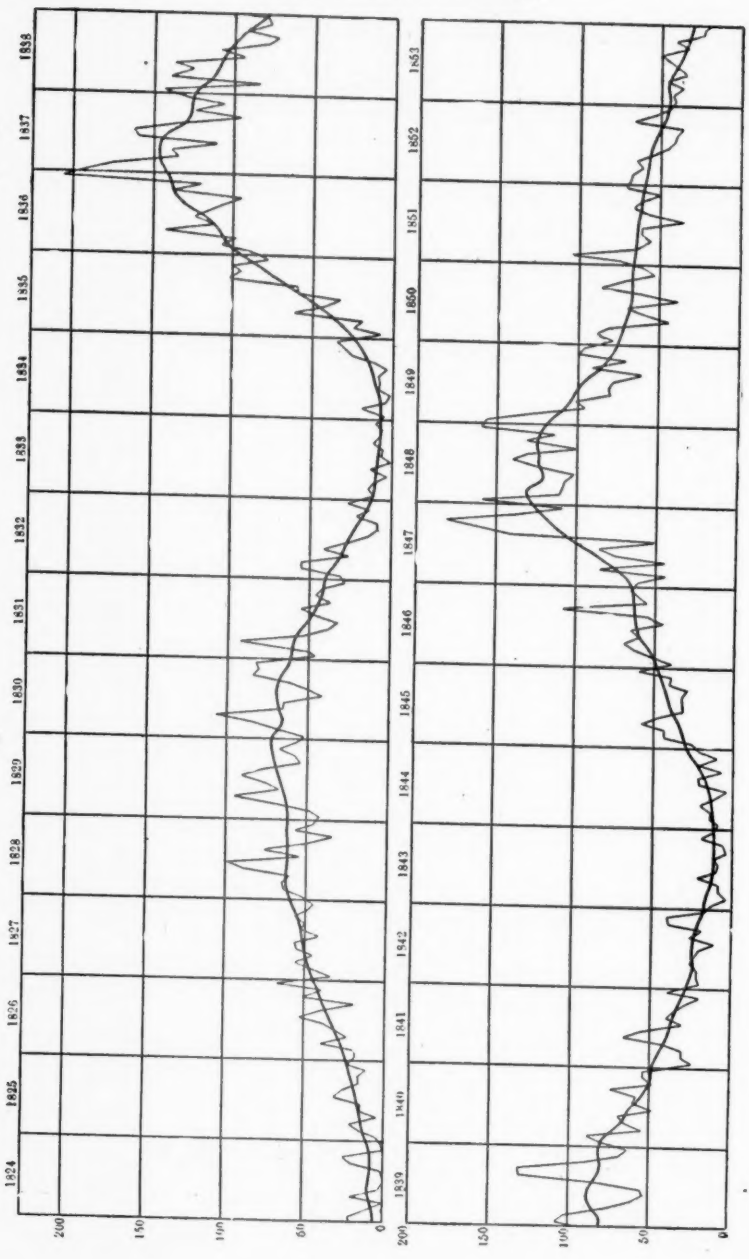
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TABLE 3.—*Epochs of Sunspot Maxima and Minima.*

Minima.	Weight.	Maxima.	Weight.	Minima.	Weight.	Maxima.	Weight.
1610.8	5	1615.5	2	1755.2	9	1761.5	7
1619.0	1	1626.0	5	1766.5	5	1769.7	8
1634.0	2	1639.5	2	1775.5	7	1778.4	5
1645.0	5	1649.0	1	1784.7	4	1788.1	4
1655.0	1	1660.0	1	1798.3	9	1805.2	5
1666.0	2	1675.0	2	1810.6	8	1816.4	8
1679.5	2	1685 0	2	1823.3	10	1829.9	10
1689.5	2	1693.0	1	1833.9	10	1837.2	10
1698.0	1	1705.5	4	1843.5	10	1848.1	10
1712.0	3	1718.2	6	1856.0	10	1860.1	10
1723.5	2	1727.5	4	1867.2	10	1870.6	10
1734.0	2	1738.7	2	1878.9	10	1883.9	10
1745.0	2	1750.3	7	1889.6	10	1894.1	10

	Year.
Mean date of minima	1744.21 ± 0.30
Mean date of maxima	1749.37 ± 0.43
Length of the period from the minima	= 11.141 ± 0.036
Length of the period from the maxima	= 11.091 ± 0.053
Length of the period in general, or 1/3 (max. + 2 min.)	= 11.124 ± 0.030

TABLE 4.—*Mutual Relations of Maxima and Minima.*

Date of		Interval		Ratio $a:b$.	$r_{max.}$	Σr, min. to min.	Length of period P, min. to min.	$r_{mean} = \frac{\Sigma r}{P}$
Min.	Max.	Max. — min. = a	Min. — max. = b					
		Years	Years				Years	
1745.0	1750.3	5.3	4.9	1.08	83	.....	.....	.....
1755.2	1761.5	6.3	5.0	1.26	80	471.6	11.3	42
1766.5	1769.7	3.2	5.8	0.55	103	533.0	9.0	59
1775.5	1778.4	2.9	6.3	0.46	151	615.8	9.2	67
1784.7	1788.1	3.4	10.2	0.33	133	840.9	13.6	62
1798.3	1805.2	6.9	5.4	1.28	47	281.6	12.3	23
1810.6	1816.4	5.8	6.9	0.84	46	238.0	12.7	19
1823.3	1829.9	6.6	4.0	1.65	67	394.7	10.6	37
1833.9	1837.2	3.3	6.3	0.52	137	653.9	9.6	68
1843.5	1848.1	4.6	7.9	0.58	125	691.8	12.5	55
1856.0	1860.1	4.1	7.1	0.58	95	548.7	11.2	49
1867.2	1870.6	3.4	8.3	0.41	132	617.3	11.7	53
1878.9	1883.9	5.0	5.7	0.88	65	382.8	10.7	36
1889.6	1894.1	4.5	*7.4	*0.61	84	*458.8	*11.9	*39

* These figures depend on assumed values of the date and the r for the minimum of 1901.5.

THE CASE OF THE LEONIDS.

JOHN B. WOOD.

FOR POPULAR ASTRONOMY.

There are two propositions which will be defended:

1. Showers of Bielids have occurred not at "returns" of the comet.

2. On the analogy of the Bielids the Leonid phenomena can be entirely accounted for without throwing the labors of Adams, Newton and Schiaparelli in the waste basket.

And first as to the Bielids: No philosopher has said that 13 added to 1872 makes 1885. Nor has he added 7 to 1892 or to 1885. Nobody has remarked that twice 6½ is 13, or that three times 6⅔ fits 1892 (from 1872).

The echoes of such declarations have not stirred the meteoric silence. Why?

Well, because prediction in *this* case is not as easy as aftersight. for it is very evident that neither 7, 6½ nor 6⅔ will fit the whole circle of the facts. In other words the off hand arithmetic of the man in the street "don't go" in the case of the Bielids.

Now suppose one does this: Assumes 6.62 to be the Bielid period. Starts from the fact that the shower mass of Nov. 27, 1872 was (say) 80 days behind the comet (of 1832). Then spaces off (in time) on the orbit of Biela thus:

(*a*.) The shower mass of 1885, 7 days ahead of
(*b*). That of Comet 1832, 80 days ahead of
(*c*). That of 1872, 41 days ahead of
(*d*). That of 1892, 139 days ahead of
(*e*). That of 1899.

Besides the twin Biela and Pogson's mass of Dec. 3, 1872. If both—making 7 clusters. In such an arrangement 3.6525 days is the one hundredth of a year.

No exactitude is claimed. It however comes out that an approximate 6.62 is all we need to account for the several showers, and that separate clusters or places in a stream are necessary to fit the facts. Agreeable, be it observed, to the primary phenomenon of the origin of the twins, in 1846.

The Leonids follow the analogy of Biela. We maintain the period of 33¼ and satisfy the historical requirements. Let this now be shown. Let a tape line represent the stream of meteors or succession (in time) of clusters in the orbit. Each inch is one-quarter of a year. Four inches in a year. Begin with 1865 to the left, the motion is from right to left; *i. e.*, the meteors arrive

at the critical place on the Earth's orbit, in succession from right to left. Then 1866 is at 4, 1867 at 8, 1868 at 12, etc. Now at the date 1866 where was the set or place giving the meteors of 1833? As the period is 33¼ and as only 33 years have elapsed, the place must be one-fourth year yet to arrive. Hence the place is at 5. Similarly we find the various years in the table below, which gives the sequence from left to right; each step, so to speak, being one-fourth of a year, in time, on the orbit.

The years of fairly interesting showers are indicated by "s". It is to be noted that each year corresponds to the one 133 prior to it; because 4 times 33¼ is 133. A question is made at 1766 because the *day* is not ascertained.

From simple inspection the chances are against a shower in 1902, which would lie to the right three numbers from 1868.

There is nothing in this way of considering the phenomena which materially disagrees with the results of Messrs. Stoney and Downing. The meteors may have been sparsely scattered in regions 1899, 1900, 1901; or may, previously, have been disturbed by Jupiter and Saturn so as to lie within or without the average orbit by enough to miss the Earth near perihelion.

TABLE OF LEONID PHENOMENA.

		S	s?	S	S			S				S	
1865	1832	1799	1766	1866	1833	1800	1900	1867	1834	1801	1901	1868	1835
			1899										

RIVERSIDE, Calif.

A SEARCH FOR AN INTRA-MERCURIAL PLANET AT THE TOTAL SOLAR ECLIPSE OF 1901, MAY 18.

C. D. PERRINE.

The considerable motion of the line of apsides of Mercury's orbit, found by Leverrier and others, cannot be explained by the attraction of the known bodies in the system, but would be fully accounted for by a planet or ring of planets between Mercury and the Sun.

The question is one which has attracted more or less attention from astronomers for over a century, and announcements have been made at various times of the supposed discoveries of such bodies. None of these announcements have ever been verified, however.

The solution of the problem is of peculiar difficulty. The necessary observations are possible only during the few minutes of a total eclipse, when the Sun's disk is entirely hidden. At any other time it would be impossible to detect an object very much fainter than Mercury.

The early searches at total eclipses were all made visually, and it was necessary to memorize the position and brightness of all the brighter stars in the region to be examined. The liability to mistake in the identification of stars was thus very great. Several of the supposed discoveries are probably due to this cause.

Searches by visual methods at a number of eclipses rendered it almost certain that there were no such bodies as bright as the third, and possibly the fourth, magnitude. But as to fainter objects there was no evidence whatever.

Since the introduction of the rapid dry-plate into astronomical work, it has become possible to attempt an adequate search for such bodies.

The first attempt, to my knowledge, to solve this problem with cameras of considerable focal length was during the eclipse of May 28, 1900. No definite results seem to have been obtained at that eclipse, however.

The great duration of totality (6½) minutes) in 1901 was very favorable for an intra-Mercurial planet search. The observations made in Sumatra for this purpose were described in the account ot the Crocker Expedition from the Lick Observatory, published in No. 81 of the *Publications*. A re-examination of these plates has just been completed under more favorable conditions and has resulted in finding many fainter stars and some star-images on all of the plates.

Following is a summary of the results of the examination:—

No. of Plates.	Star Images.			Remarks.
	Original Search.	Added by Re-examination.	Total Number.	
A_1 B_1	29	21	50	Faintest stars = one each of 9.1, 9.2 and 9.3 visual mag.
C_1 D_1	37	14	51	Faintest stars = two each of 8.6 and 9.0 visual mag.
A_2 B_2	26	9	35	Faintest stars = one each of 8.7 and 8.8 visual mag.
C_2 D_2	0	26	26	Faintest stars = three of 8.3 1 each of 8.6 & 8.7 vis. mag.
A_3 B_3	0	3	3	One star each of 6.0, 6.2 and 6.5 vis. mag. Mercury and Venus are bright objects on the plates.
C_3 D_3	0	5	5	One star each of 4.0, 5.2 and 6.5 vis. mag. and two of 6.0 visual magnitude.
Total	92	78	170	

Four out of the six regions covered by the photographs show stars from 8.7 to 9.3 visual magnitude. These were the first two sets of exposures. The clouds became thicker toward the end of totality, so that the set of exposures made then fails to show as faint stars. They, however, contain stars from 6.0 to 6.5 magnitude. Only two stars in the entire region, which were brighter than 6.0 visual magnitude, failed to record themselves on all of the plates. One of these was 5.0, the other 5.1 visual magnitude. According to the Draper Catalogue, the photographic magnitudes of these stars are 5.2 and 6.1, the former being a red star.

As the faintest stars recorded on the negatives are those which, in general, are more strongly actinic than a planetary body, it is necessary to make some allowance for this difference in deducing the limiting (visual) magnitude of the search. It seems probable that this difference would usually not exceed one magnitude. Hence if all the stars of 6.0 visual magnitude were recorded on the plates, a planet of 5.0 magnitude should be visible. The same criterion would indicate that in two-thirds of the region searched where stars of 8.7 to 9.3 magnitude were photographed, a planet as bright as 7¾ or 8.0 magnitude should have been recorded.

There is therefore little probability that any planet as bright as 5.0 magnitude exists in the region searched. While the clouds prevented the search from being definite to a much fainter magnitude, it is hardly possible that a ring of planets as bright as 8th magnitude can exist without some of them having been found.

From observations of the brightness of the four principal asteroids made at Harvard College Observatory and the measures of their diameters made by Barnard at the Lick Observatory, it is found that such a body 300 miles in diameter appears on the average as a star of the 8.0 magnitude when seen from the Earth at opposition. If such a body were removed to the greatest probable distance from the Sun of an intra-Mercurial planet, it would appear to an observer on the Earth more than 110 times as bright, a difference of over five magnitudes. Hence we find that an intra-Mercurial planet to be of 5.0 magnitude would have to be only 75 miles in diameter, and one of 8.0 magnitude but 30 miles in diameter.

The perturbations in the motion of Mercury would require a mass of matter about half as great as that of the planet itself. It would therefore take one million intra-Mercurial planets 30 miles in diameter, and as dense as Mercury, to cause the observed disturbances.

The observations made at this eclipse, although very much

interfered with by the clouds, indicate that there are probably no bodies of appreciable size in the region close about the Sun, and that the cause of the disturbance in the motion of Mercury must be sought elsewhere.

The suggestion has also been made that these perturbations might be caused by a ring of bodies between Mercury and Venus. If this were true, it would seem that some effect should be observed in the motion of Venus.

Perhaps the finely divided matter composing the zodiacal light may be sufficient to cause these perturbations.

Publications of the Astronomical Society of the Pacific,
Volume XIV, No. 86, 1902.

IS A NEW SYSTEM OF MOUNTING A LARGE TELESCOPE TO BE ADOPTED AT AMHERST?

S. B. ELLIOTT.

FOR POPULAR ASTRONOMY.

The announcement in the August and September number of POPULAR ASTRONOMY that Amherst is to have a new Observatory and that Professor Todd has secured a first-class 18-inch objective for a new telescope was very gratifying to all interested in astronomy.

From the account given it appears that the telescope is to be mounted in a dome 33 feet in diameter and we are told that "Professor Todd, a year or two ago, spent several weeks in studying appliances and mechanism of continental observatories and will embody the results of his investigations, together with numerous inventions of his own."

The last clause of this quotation is, to me, the most interesting part, for there was published in the *New York Tribune*, some six or more months ago, an illustrated article descriptive of an unique mounting for a large telescope and Professor Todd was credited with being its inventor, and only a few weeks ago Miss Mary Proctor gave, in the *Scientific American*, a similar description, with illustrations, of Professor Todd's device. According to these accounts this consists of a huge hollow metal sphere, out of which the object-glass end of the telescope protrudes one-half its length. The eye-piece end extends inwardly to near the opposite side of the shell where the observer will sit in a fixed comfortable seat. Of course a door, for his use, would lead to the interior. If desired about one-half of the shell could be left open or covered with glass or sliding shutters. This big ball is to float in some fluid, water, oil, or mercury and of course, is to

be properly balanced so that the telescope can be readily pointed to any part of the heavens—a sort of "ball joint." The tube in addition to being firmly fastened to the shell, is rigidly braced to prevent springing.

This for the scheme looks fairly well on paper; but how is this big globe to be moved, how held in place after it is moved, and how made to accurately adjust itself to the rotation of the Earth? True, there is shown in the illustrations a shaft with gears at one end and a crank at the other, but no device appears to connect these with the ball, and I confess my utter inability to conceive how this can be done in a way that will permit it to be moved in any direction required, nor how or where the hour and declination circles can be placed and the whole under the proper guidance of clock work.

But suppose the difficulties I name can be overcome, there are still others which I think will have to be dealt with. To bring about accuracy of movement nearly one-half of the globe must be, that much of it, a perfect sphere, and how can such be constructed, or, if constructed, how kept so? The varying temperature that the exposed part will experience, and which cannot be promptly shared by the submerged portion, will most certainly so distort its form as to render the accurate adjustment of any instrument connected with it very unstable, for as the tube is rigidly fixed to the hollow sphere, it must partake of the same movements.

Then, too, what will prevent the wind from causing such a tremor of the telescope as will be fatal to any observation? No shield for the outer end of the telescope can be provided, as there is in the revolving dome, nor can the sphere itself—I think 50 feet was the diameter named—escape from the effects of even a very moderate breeze. All readings must be taken by the assistant who must, necessarily, be posted on the outside of the shell, and where he will be placed is a wonder to me. However, this may not be one of the "numerous inventions" spoken of which Professor Todd proposes to adopt at Amherst. Yet the statement that the dome is to be 33 feet in diameter and the length of the tube 14 feet—this may be a typographical error—gives coloring to the supposition that a radical departure from usual methods is, somehow, to be made. I shall watch, with great interest any attempt—if it shall be made—to make a floating ball a satisfactory mounting for a large telescope—or a small one, for that matter.

REYNOLDSVILLE, Pa.

TWO IMPORTANT BOOKS.

W. W. PAYNE.

The first of these books to which reference is made is the new edition of the History of Astronomy during the Nineteenth Century, by Agnes M. Clerke, and published July 1902, by Adam and Charles Black, London, England and also by The Macmillan Company, 66 Fifth Ave., New York; pp. 489; price $4.00.

The first edition of this well-known publication appeared in 1885. It then received marked attention generally, because of its strong title, and the place it sought to fill. The History of Physical Astronomy by Professor Grant, published a third of a century earlier had easily held the first place in the minds of those competent to judge of the merits of such books, but wonderful progress had been made in the science of astronomy during a period of thirty years, so the task of taking up the history where Grant left it, and of carrying it on through the nineteenth century was one that the most scholarly in the science from any country would hesitate to undertake. Not only so on account of the standard character of the work it supplements, but also especially because of the difficult and very complex nature of the various new branches of astronomy that are yet but a few years old, but which are showing fruits that fill the minds of the best scholars with mingled uncertainty and amazement.

The astronomer of today knows, as it was impossible for any to know in 1885, what a momentous task it was then to collect data and use them for a reliable history. But Miss Clerke did that work well under those most trying circumstances, and the best judges in America have freely accorded her the honor due for the achievement of a result that is a fitting sequel to that of Professor Grant.

If Grant's book was not revised it is no fault of the work of Miss Clerke that it should have passed into the fourth edition in less than twenty years. The same thing is true of modern astronomy in every line. One of the ablest astronomers in America wrote a college text-book in 1888 and in ten years he revised it making more changes in the particular line of his own specialization than in any other. That course was right, because that line of study had advanced rapidly and the latest results should find place in a truly representative text-book. The same thing must be said of a history of physical astronomy written in this very changeful period now under consideration. It is of interest

to our readers to compare, very briefly, the first and fourth editions of this history, in order to have a fuller idea of the advancement made in the science in seventeen years.

In the first chapter that treats of the foundation of sidereal astronomy there are no important changes. In the next, which consider the progress of sidereal astronomy the corrections and additions are considerable. This work of modification begins on page 38, following which almost every paragraph in the chapter is essentially changed. The author seeems to have been careful to include all that is new and to have gone over thoroughly the old for the necessary revision to put all in harmony. The last half of chapter 3 which treats of recent solar eclipses has been modified in a similar way.

Almost every one of the remaining ten chapters have been recast to adapt them to the present state of knowledge of the several topics of which they severally treat. There has been added to this edition an appendix of twenty-seven pages which is new. It consists of a table of chronology of astronomical events from 1774, March 4, Herschel's first observation of the nebula of Orion, to 1901, the last sunspot minimum; a table of chemical elements in the Sun, determined by Professor Rowland bringing the investigation down to 1901. This table is arranged to show comparatively the number of representative lines of the elements also indicating those which are doubtful and naming those not found in the Sun. Table 3 gives the epochs of sunspot maximum and minimum to the year 1901. Those interested in this theme will likely compare this table with the data found in the leading article of this publication. Table 4 has to do with the movements of the Sun and stars: 1. The translation of the solar system; 2. Stellar velocities. Table 5 gives a list of great telescopes in the world. 1. Reflectors with metallic specula; 2. Reflectors with mirrors of silvered glass and 3, A list of large refractors including all known at the date of writing above 14.6 inches clear aperture. This table gives the place of the instrument, its aperture in inches, its focal length, its maker, and a column of remarks. Table 6 furnishes a list of observatories employed in the construction of the photographic chart and catalogue of the heavens which is now going on. It is one of the greatest, if not the greatest, piece of astronomical work known to the science of astronomy.

From what has been said our readers who have not seen this book will have some idea of its usefulness to those who wish to know what is the present condition of astronomy, today, in the

line of the advance guard of its practical workers. So far as we know, there can not be found elsewhere within the limits of one book of less than 500 pages so much information scientifically correct, yet adapted to popular reading as this book contains.

The other book referred to above, is a new text-book and bears the title "An Introduction to Celestial Mechanics." It is written by F. R.Moulton, Ph. D., instructor in astronomy in the University of Chicago, and published by The Macmillan Company of New York, 1902.

The plan of the author in this new book is to give a brief but exact account of several parts of Celestial Mechanics rather than a full and more detailed treatment of any special part. As far as we have been able to examine the book, somewhat thoroughly, this plan has been carried out well. It has unity and logical sequence and is progressively difficult in plain subject matter. This gives it great scientific value and educational worth for the necessary training of young mathematicians who want to lay a strong foundation in mathematics for that most delightful branch of astronomy known as Celestial Mechanics.

It is not an easy task for the author to select from the great variety of standard works on the kindred branches of astronomy which specialize so fully as to make a large library of ponderous volumes, and use only that matter which would be suitable for an introductory volume, for a student who has some knowledge of calculus, and must work independently for further preparation. He has not given the relative prominence to some themes in this branch that it was the fashion to do twenty years ago; for example, the large place that was given to the theory of orbits, and the time required for the computation of common and special cases. This gives the student excellent practice, but if introduced too early in the course of his preparation, it will be unnecessarily tedious work. It is easy to see how this change in the plan of study has come about, and why the author is led to adopt it. There are now so many new things pressing upon the attention of the practical astronomer, from the side of modern physical research, that require the use of the higher mathematics, that the mathematical astronomer must give them attention. Indeed he has been doing so, very largely in recent years. The theory of the spectroscope and the interpretation of its results have been a most prolific field. The alert and skillful spectroscopist of today does not very well know what that wonderful instrument can do. The same may be said of photography. These astrophysical branches and others akin to them are urging the mathema-

tician to adopt their powers, interpret their results, exclude their errors, and bring out their observations into a consistent whole that they serve for generalization and possibly for important discovery.

This new book takes notice of these things and therefore forms a most helpful link in the chain of endeavor to do needful scientific work quickly, logically and successfully.

When we first opened the book, it appeared to be so much like the one we have been wanting for years, that we were anxious to know its contents and read the first thirty pages immediately. The first chapter deals with fundamental principles and definitions, the nature of the laws of motion, acceleration, application of motion to the ellipse, application to a particle moving in a circle, the areal velocity, the center of mass of n equal particles, the center of gravity, center of mass of a continuous body, planes and axes of symmetry, application to a heterogenious cube, to the octant of a sphere, and problems on the center mass. Then follows a historical sketch from ancient times to Newton, the two divisions of the history, formal astronomy and dynamical astronomy, closing the chapter with nearly a page of bibliography.

The other chapters are treated on the same general plan. The theme of the second is rectilinear motion; 3rd, central forces; 4th, the potential and attraction of bodies; 5th, the problem of two bodies; 6th, the general integral of the problem of n bodies; 7th, the problem of three bodies; 8th, perturbations—geometrical considerations; 9th, perturbations—analytical methods; 10th, theory of the determination of the elements of parabolic orbits; 11th, theory of the determination of the elements of elliptic orbits.

This book contains 384 pages, is printed in two sizes of clear type, the chapters are divided into sections which are numbered and provided with side-head titles in heavy faced type. The geometrical proofs are accompanied by figures which materially aid in following the transformations of the mathematical text. The mathematical notation is in first rate order and of the best modern style. The proofs, as far as examined, are rigorous in the method of limits and therefore especially satisfactory.

The other strong features of the book are found in the make-up of the matter of the chapters. The lucid demonstration, using the geometrical, analytic or calculus method of proof as is best for the particular case in hand; the illustrative examples for the exercise of the reader and finally the historical sketches and

bibliography. It seems to us that the author has done his work admirably. The text will be used in Carleton College for advanced classes in mathematics and astronomy.

LIQUEFACTION OF GASES AND LOW TEMPERATURES.

[The following extract from the inaugural address by Professor James Dewar President of the British Association, at its Belfast meeting, treats so fully and well of important themes relating to the progress of astronomy that space will be given to it in this and following numbers of this publication.—EDITOR.]

I have thought that it might be instructive, and perhaps not uninteresting to trace briefly in broad outline the development of that branch of study with which my own labors have been recently more intimately connected—a study which I trust I am not too partial in thinking is as full of philosophical interest as of experimental difficulty. The nature of heat and cold must have engaged thinking men from the very earliest dawn of speculation upon the external world; but it will suffice for the present purpose if, disregarding ancient philosophers and even medieval alchemists, we take up the subject where it stood after the great revival of learning, and as it was regarded by the father of the inductive metnod. That this was an an especially attractive subject to Bacon is evident from the frequency with which he recurs to it in his different works, always with lamentation over the inadequacy of the means at disposal for obtaining a considerable degree of cold. Thus in the chapter in the Natural History, "Sylva Sylvarum," entitled "Experiments in consort touching the production of cold," he says, "The production of cold is a thing very worthy of the inquisition both for the use and the disclosure of causes. For heat and cold are nature's two hands whereby she chiefly worketh, and heat we have in readiness in respect of the fire, but for cold we must stay till it cometh or seek it in deep caves or high mountains, and when all is done we cannot obtain it in any great degree, for furnaces of fire are far hotter than a summer Sun, but vaults and hills are not much colder than a winter's frost." The great Robert Boyle was the first experimentalist who followed up Bacon's suggestions. In 1682 Boyle read a paper to the Royal Society on "New Experiments and Observations touching Cold, or an Experimental History of Cold," published two years later in a separate work. This is really a most complete history of everything known about cold up to that date, but its great merit is the inclusion of numerous experiments made by Boyle himself on frigorific mixtures,

and the general effects of such upon matter. The agency chiefly used by Boyle in the conduct of his experiments was the glaciating mixture of snow or ice and salt. In the course of his experiments he made many important observations. Thus he observed that the salts which did not help the snow or ice to dissolve faster gave no effective freezing. He showed that water in becoming ice expands by about one-ninth of its volume, and bursts gun barrels. He attempted to counteract the expansion and prevent freezing by completely filling a strong iron ball with water before cooling; anticipating that it might burst the bottle by the stupendous force of expansion, or that if it did not, then the ice produced might under the circumstances, be heavier than water. He speculated in an ingenious way on the change of water into ice. Thus he says, "If cold be but a privation of heat through the recess of that ethereal substance which agitated the little eel-like particles of the water and thereby made them compose a fluid body, it may easily be conceived that they should remain rigid in the postures in which the ethereal substance quitted them, and thereby compose an unfluid body like ice; yet how these little eels should by that recess acquire as strong an endeavor outwards as if they were so many little springs and expand themselves with so stupendous a force, is that which does not so readily appear." The greatest degree of adventitious cold Boyle was able to produce did not make air exposed to its action lose a full tenth of its own volume, so that, in his own words, the cold does not "weaken the spring by anything near so considerable as one would expect." After making this remarkable observation and commenting upon its unexpected nature, it is strange Boyle did not follow it up. He questions the existence of a body of its own nature supremely cold, by participating in which all other bodies obtain that quality, although the doctrine of a *primum frigidum* had been accepted by many sects of philosophers; for, as he says, "if a body being cold signify no more than its not having its sensible parts so much agitated as those of our sensorium, it suffices that the Sun or the fire or some other agent, whatever it were, that agitated more vehemently its parts before does either now cease to agitate them or agitates them but very remissly, so that till it be determined whether cold be a positive quality or but a privative it will be needless to contend what particular body ought to be esteemed the *primum frigidum*." The whole elaborate investigation cost Boyle immense labor, and he confesses that he "never handled any part of natural philosophy that was so troublesome and full of hardships." He

looked upon his results but as a "beginning" in this field of inquiry, and for all the trouble and patience expended he consoled himself with the thought of "men being oftentimes obliged to suffer as much wet and cold and dive as deep to fetch up sponges as pearls." After the masterly essay of Boyle, the attention of investigators was chiefly directed to improving thermometrical instruments. The old air thermometer of Galileo being inconvenient to use, the introduction of fluid thermometers greatly aided the inquiry into the action of heat and cold. For a time great difficulty was encountered in selecting proper fixed points on the scales of such instruments, and this stimulated men like Huygens, Newton, Hooke and Amontons to suggest remedies and to conduct experiments. By the beginning of the eighteenth century the freezing-point and boiling-point of water were agreed upon as fixed points, and the only apparent difficulties to be overcome were the selection of the fluid, accurate calibration of the capillary tube of the thermometer, and a general understanding as to scale divisions. It must be confessed that great confusion and inaccuracy in temperature observations arose from the variety and crudeness of the instruments. This led Amontons in 1702-3 to contribute two papers to the French Academy which reveal great originality in the handling of the subject, and which, strange to say, are not generally known. The first discourse deals with some new properties of the air and the means of accurately ascertaining the temperature in any climate. He regarded heat as due to a movement of the particles of bodies, though he did not in any way specify the nature of the motion involved; and as the general cause of all terrestrial motion, so that in its absence the Earth would be without movement in its smallest parts. The new facts he records are observations on the spring or pressure of air brought about by the action of heat. He shows that different masses of air measured at the same initial spring or pressure, when heated to the boiling point of water, acquire equal increments of spring or pressure, provided the volume of the gas be kept at its initial value. Further, he proves that if the pressure of the gas before heating be doubled or tripled, then the additional spring or pressure resulting from heating to the boiling point of water, is equally doubled or tripled. In other words, the ratio of the total spring of air at two definite and steady temperatures and at constant volume is a constant, independent of the mass or the initial pressure of the air in the thermometer. These results led to the increased perfection of the air thermometer as a standard instrument, Amon-

tons' idea being to express the temperature at any locality in fractions of the degree of heat of boiling water. The great novelty of the instrument is that temperature is defined by the measurement of the length of a column of mercury. In passing, he remarks that we do not know the extreme of heat and cold but that he has given the results of experiments which establish correspondences for those who wish to consider the subject. In the following year Amontons contributed to the Academy a further paper extending the scope of the inquiry. He there pointed out more explicitly that as the degrees of heat in his thermometer are registered by the height of a column of mercury, which the heat is able to sustain by the spring of the air, it follows that the extreme cold of the thermometer will be that which reduces the air to have no power of spring. This, he says, will be a much greater cold than what we call "very cold," because experiments have shown that if the spring of the air at boiling point is 73 inches, the degree of heat which remains in the air when brought to the freezing point of water is still very great, for it can still maintain the spring of 51½ inches. The greatest climatic cold on the scale of units adopted by Amontons is marked 50, and the greatest summer heat 58, the value for boiling water being 73, and the zero being 52 units below the freezing point. Thus Amontons was the first to recognize that the use of air as a thermometric substance led to the inference of the existence of a zero of temperature, and his scale is nothing else than the absolute one we are now so familiar with. It results from Amontons' experiments that the air would have no spring left if it were cooled below the freezing point of water to about 2½ times the temperature range which separates the boiling point and the freezing point. In other words, if we adopt the usual centennial difference between these two points of temperature as 100 degrees, then the zero of Amontons' air thermometer is *minus* 240 degrees. This is a remarkable approximation to our modern value for the same point of *minus* 273 degrees. It has to be confessed that Amontons' valuable contributions to knowledge met with that fate which has so often for a time overtaken the work of too advanced discoverers; in other words, it was simply ignored, or in any case not appreciated by the scientific world either of that time or half a century later. It is not till Lambert, in his work on "Pyrometrie" published in 1779, repeated Amontons' experiments and endorsed his results, that we find any further reference to the absolute scale of the zero of temperature. Lambert's observations were made with

the greatest care and refinement, and resulted in correcting the value of the zero of the air scale to *minus* 270 degrees as compared with Amontons' *minus* 240 degrees. Lambert points out that the degree of temperature which is equal to zero is what one may call absolute cold, and that at this temperature the volume of the air would be practically nothing. In other words, the particles of the air would fall together and touch each other and become dense like water; and from this it may be inferred that the gaseous condition is caused by heat. Lambert says that Amontons' discoveries had found few adherents because they were too beautiful and advanced for the time in which he lived.

About this time a remarkable observation was made Professor Braun at Moscow, who, during the severe winter of 1759, succeeded in freezing mercury by the use of a mixture of snow and nitric acid. When we remember that mercury was regarded as quite a peculiar substance possessed of the essential quality of fluidity, we can easily understand the universal interest created by the experiment of Braun. This was accentuated by the observations he made on the temperature given by the mercury thermometer, which appeared to record a temperature as low as *minus* 200° C. The experiments were soon repeated by Hutchins at Hudson's Bay, who conducted his work with the aid of suggestions given him by Cavendish and Black. The result of the new observations was to show that the freezing point of mercury is only *minus* 40° C., the errors in former experiments having been due to the great contraction of the mercury in the thermometer in passing into the solid state. From this it followed that the enormous natural and artificial colds which had generally been believed in had no proved existence. Still the possible existence of a zero of temperature very different from that deduced from gas thermometry had the support of such distinguished names as those of Laplace and Lavoisier. In their great memoir on "Heat," after making what they consider reasonable hypotheses as to the relation between specific heat and total heat, they calculate values for the zero which range from 1500° to 3000° below melting ice. On the whole, they regard the absolute zero as being in any case 600° below the freezing point. Lavoisier, in his "Elements of Chemistry" published in 1792, goes further in the direction of indefinitely lowering the zero of temperature when he says, "We are still very far from being able to produce the degree of absolute cold, or total deprivation of heat, being unacquainted with any degree of coldness which we

cannot suppose capable of still further augmentation; hence it follows we are incapable of causing the ultimate particles of bodies to approach each other as near as possible, and thus these particles do not touch each other in any state hitherto known." Even as late as the beginning of the nineteenth century we find Dalton, in his new system of "Chemical Philosophy," giving ten calculations of this value, and adopting finally as the natural zero of temperature *minus* 3000° C.

In Black's lectures we find that he takes a very cautious view with regard to the zero of temperature, but as usual is admirably clear with regard to its exposition. Thus he says, "We are ignorant of the lowest possible degree or beginning of heat. Some ingenious attempts have been made to estimate what it may be, but they have not proved satisfactory. Our knowledge of the degrees of heat may be compared to what we should have of a chain the two ends of which were hidden from us and the middle only exposed to our view. We might put distinct marks on some of the links, and number the rest according as they are nearest to or further removed from the principal links; but not knowing the distance of any links from the end of the chain we could not compare them together with respect to their distance, or say that one link was twice as far from the end of the chain as another." It is interesting to observe, however, that Black was evidently well acquainted with the work of Amontons, and strongly supports his inference as to the nature of air. Thus, in discussing the general cause of vaporisation, Black says that some philosophers have adopted the view "that every palpable elastic fluid in nature is produced and preserved in this form by the action of heat. Mr. Amontons, an ingenious member of the late Royal Academy of Sciences, at Paris, was the first who proposed this idea with respect to the atmosphere. He supposed that it might be deprived of the whole of its elasticity and condensed and even frozen into a solid matter were it in our power to apply to it a sufficient cold; that it is a substance that differs from others by being incomparably more volatile, and which is therefore converted into vapor and preserved in that form by a weaker heat than any that ever happened or can obtain in this globe, and which therefore cannot appear under any other form than the one it now wears, so long as the constitution of the world remains the same as at present." The views that Black attributes to Amontons have been generally associated with the the name of Lavoisier, who practically admitted similar possibilities as to the nature of air; but it is not likely that in such

matters Black would commit any mistake as to the real author of a particular idea, especially in his own department of knowledge. Black's own special contribution to low temperature studies was his explanation of the interaction of mixtures of ice with salts and acids by applying the doctrine of the latent heat of fluidity of ice to account for the frigorific effect. In a similar way, Black explained the origin of the cold produced in Cullen's remarkable experiment of the evaporation of ether under the receiver of an air-pump by pointing out that the latent heat, of vaporisation in this case necessitated such a result. Thus, by applying his own discoveries of latent heat, Black gave an intelligent explanation of the cause of all the low-temperature phenomena known in his day.

After the gaseous laws had been definitely formulated by Gay-Lussac and Dalton, the question of the absolute zero of temperature, as deduced from the properties of gases, was revived by Clement and Desormes. These distinguished investigators presented a paper on the subject to the French Academy in 1812, which, it appears, was rejected by that body. The authors subsequently elected to publish it in 1819. Relying on what we know now to have been a faulty hypothesis, they deduced from observations on the heating of air rushing into a vacuum the temperature of *minus* 267 degrees as that of the absolute zero. They further endeavored to show, by extending to lower temperatures the volume or the pressure coefficients of gases given by Gay-Lussac, that at the same temperature of *minus* 267 degrees the gases would contract so as to possess no appreciable volume, or alternatively, if the pressure was under consideration, it would become so small as to be non-existent. Although full reference is given to previous work bearing on the same subject, yet, curiously enough, no mention is made of the name of Amontons. It certainly gave remarkable support to Amontons' notion of the zero to find that simple gases like hydrogen and compound gases like ammonia, hydrochloric, carbonic and sulphurous acids should all point to substantially the same value for this temperature. But the most curious fact about this research of Clement and Desormes is that Gay-Lussac was a bitter opponent of the validity of the inferences they drew either from his work or their own. The mode in which Gay-Lussac regarded the subject may be succinctly put as follows: A quick compression of air to one-fifth volume raises its temperature to 300 degrees, and if this could be made much greater and instantaneous the temperature might rise to 1000 or 2000 degrees.

Conversely, if air under five atmospheres were suddenly dilated, it would absorb as much heat as it had evolved during compression, and its temperature would be lowered by 300 degrees. Therefore, if air were taken and compressed to fifty atmospheres or more, the cold produced by its sudden expansion would have no limit. In order to meet this position, Clement and Desormes adopted the following reasoning: They pointed out that it had not been proved that Gay-Lussac was correct in his hypothesis, but that in any case it tacitly involves the assumption that a limited quantity of matter possesses an unlimited supply of heat. If this were the case, then heat would be unlike any other measurable thing or quality. It is, therefore, more consistent with the course of nature to suppose that the amount of heat in a body is like the quantity of elastic fluid filling a vessel, which, while definite in original amount, one may make less and less by getting nearer to a complete exhaustion. Further, to realize the absolute zero in the one case is just as impossible as to realize the absolute vacuum in the other; and as we do not doubt a zero of pressure, although it is unattainable, for the same reason we ought to accept the reality of the absolute zero. We know now that Gay-Lussac was wrong in supposing the increment of temperature arising from a given gaseous compression would produce a corresponding decrement from an identical expansion. After this time the zero of temperature was generally recognized as a fixed ideal point, but in order to show that it was hypothetical a distinction was drawn between the use of the expression, zero of absolute temperature and the absolute zero.

The whole question took an entirely new form when Lord Kelvin, in 1848, after the mechanical equivalent of heat had been determined by Joule, drew attention to the great principles which underly Carnot's work on the "Motive Power of Heat," and applied them to an absolute method of temperature measurement, which is completely independent of the properties of any particular substance. The principle was that for a difference of one degree on this scale, between the temperatures of the source and refrigerator, a perfect engine should give the same amount of work in every part of the scale. Taking the same fixed points as for the centigrade scale, and making 100 of the new degrees cover that range, it was found that the degrees not only within that range, but as far beyond as experimental data supplied the means of comparison, differed by only minute quantities from those of Regnault's air thermometer. The zero of the new scale had to be determined by the consideration that when the refriger-

ator was at the zero of temperature the perfect engine should give an amount of work equal to the full mechanical equivalent of the heat taken up. This led to a zero of 273 degrees below the temperature of freezing water, substantially the same as that deduced from a study of the gaseous state. It was a great advance to demonstrate by the application of the laws of thermodynamics not only that the zero of temperature is a reality, but that it must be located at 273 degrees below the freezing point of water. As no one has attempted to impugn the solid foundation of theory and experiment on which Lord Kelvin based his thermodynamic scale, the existence of a definite zero of temperature must be acknowledged as a fundamental scientific fact.

NEW COMET *b* 1902 (PERRINE.)

W. W. PAYNE.

The new comet, which has received general attention lately, was discovered by C. D. Perrine, of Lick Observatory, Mount Hamilton, California, Sept. 1. It was a faint object at the time of discovery. In the telescope, its appearance was that of a nebula with a small, somewhat well-defined nucleus, near the center of the head, but not exactly so, and its brightness was about that of an eleventh magnitude star. The size of the coma was about five minutes in diameter, and from the first the new comet had a short brushy tail about ten minutes long pointing towards the southwest. At any time it is very difficult to determine the length of the tail of a comet, because it gradually diminishes in brightness, as it extends away from the head, until it is so very faint that the observer can not tell the place of its termination if it has any. In trying to estimate the length of the train of this comet. the writer thought sometimes, by indirect vision, that the tail was fully twice as long as it appeared by direct vision. Not having any photograph of the object at the time of this observation, it was wholly uncertain which view is the more correct one.

Mr. Perrine took three observations of the comet Aug. 31, Sept. 1 and Sept. 2, and from them he computed parabolic elements of an orbit which show interesting features, regarding its place in the solar system, the direction of its rapid motion, its brightness and its period of visibility. He also computed an ephemeris of it, covering a period from Sept. 6 to Nov. 23, which was published in the last number of this magazine.

From the *Astronomische Nachrichten*, No. 3812, we learn that

Elis Strömgren of Kiel, Germany, also computed an orbit of the comet, using one observation made at Lick Observatory Sept. 1, one made at Urania Sept. 2, and the third made at Copenhagen Sept. 4. From these three observations obtained from three different places, Mr. Strömgren obtains nearly the same results as those given by Mr. Perrine from observations made entirely at the Lick Observatory. The agreement is so close that a good degree of confidence is felt in the fact that these trial orbits must be close approximations to the true path of the comet which will be known later when a longer series of observations have been made for the purpose of computing a definitive orbit.

So far as we have been able to learn this interesting comet is entirely new. Of this we can not yet be entirely certain, until the character of the orbit is more definitely known. One astronomer has suggested that the new comet is possibly the return of an old one, but of this he does not feel certain enough to hazard a prediction.

From the orbits thus far computed the range of brightness from the time of discovery to the early part of November is very great. Through the month of September its light increased very rapidly. At the time of discovery its brightness was indicated by the figure 1, that meant in Mr. Perrine's estimate a light equal to that of a 9th magnitude star. On Sept. 5th its brightness was 1.5; on 10th, 2.10; on 14th, 3.02; on the 18th, 4.52. On Oct. 4th its light was predicted to be 25.45, Oct. 6th, 29.18. On Nov. 8th to be 13.9, but on Nov. 23, 17.2. This change of brightness is due to the changes of distance from the Earth and from the Sun, and it is calculated according to the law of the square of distances taken inversely. On the evening of Oct. 7, the comet was observed at Goodsell Observatory and its brightness was estimated as about the same as that of two stars near by in Cygnus whose magnitudes are catalogued as nearly of the fourth. If Mr. Perrine's estimate at the time of discovery was correct, the comet's brightness had increased five magnitudes which would make it one hundred times as bright as when it was first seen. The ephemeris puts it less than thirty times as bright as it was at the time of the first observation. If we were to say that the comet had increased its brightness only by four magnitudes, making large allowance for the difficulty met in trying to compare the brightness of a comet with that of a neighboring star, the light of the comet on Oct. 7 would stand at 39.80 instead of 28.61.

Observers very well know that comets approaching the Sun do

not follow closely the law of the square of inverse distances, for the change of brightness, yet this table of values is very useful in the study of the real physical changes in the comet that are liable to take place in the neighborhood of the Sun. For this reason careful attention may be given to the changes in its light to detect any capricious behavior that in some comets has been observed to be unaccountably and astonishingly wild. Frequent comparisons with neighboring stars is a good test and the opera-glass may do excellent service in taking such observations, because the work done can be done so quickly and so well.

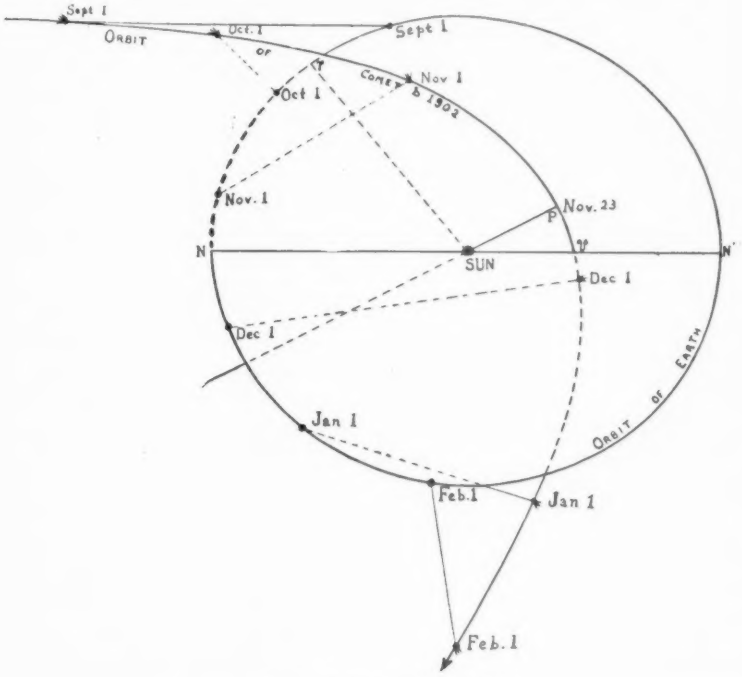

Diagram of Comet *b*, 1902.
Drawn by F. R. Eastman.

From the accompanying cut the path of the comet relative to that of the Earth is shown and the positions of both bodies given that their relative distances at the times mentioned may be seen at a glance. At the time of discovery it was moving almost directly towards the Earth, but before it reached the orbit of the Earth our planet had passed on beyond the point opposite, so

that collision would have been impossible provided the paths of both bodies were lying in the same plane; but as they are not in the same plane the points of intersection are such that there could not possibly be any such danger even if their approach had been less than it was.

Other interesting features shown by this preliminary path of the comet are its near approach to the Sun, its long period of visibility, its short time behind, or on the opposite side from, the Sun, its near approach to the Earth again in the months of January and February 1903. If the computers have given us a path for the comet that is nearly correct, its brightness in approaching the Earth the second time will not be nearly as great as that which was observed during the months of September and October of this year. But on Jan. 16, the time of nearest approach, the brightness will be less than six times that of discovery. These predictions do not take into account any of those physical changes which so often take place when a comet is passing near the Sun especially if its perihelion point falls far within the Earth's orbit as is true of this comet. It is fortunate that this comet has so long a period of visibility that it may be observed thoroughly to determine a definitive orbit and to make a thorough study of its physical changes.

The photographic reproduction herewith presented should show two trains instead of one. The negative from which the print for reproduction was made, plainly shows both tails. They both extend away from the Sun and have an angle of less than thirty degrees between them. The fainter one, from its position and its straightness seems to be of the hydrogen type. There is some difficulty in estimating the length of the tails because of the many star trails that fill the field of the negative. There are indications that the length of the hydrogen train is three or four times that which is plainly shown. But of this we can not be certain.

TOTAL ECLIPSE OF THE MOON OCTOBER 16, 1902.

W. W. PAYNE.

The evening of Oct. 16 was cloudy at Northfield and there was little hope of seeing the eclipse which was predicted for that night. However, at 9 o'clock the atmospheric conditions were somewhat improved, so preparations were hastily made for some observations that might be taken if opportunity for the same could be had.

The time for contact of the penumbra was, in Central time reckoning 9^h 17.1^m P. M., and that for the contact of the shadow called the umbra at 10^h 17.3^m P. M. About this time the clouds were more broken and moving rapidly so that it was possible to see the Moon fairly well every few minutes. The darkening of the Moon's limb at the point of contact was noticeable a few minutes before the predicted contact of the shadow. This, of course, was due to the penumbra in which the Moon had already been immersed for about one hour. By the aid of a five-inch telescope the phase of the eclipse was watched as closely as possible, in the presence of passing clouds. The time that we noted for contact of the umbra was 10^h 17^m 40^s. This estimate can only be probable because of the indefinite edge of the umbra; it is surrounded by a nebulous border which is plainly seen in the telescope, but not so noticeable in a naked-eye view. As the shadow came on to the Moon it was black, and continued to be so until it covered about one-half of the lunar disk. From this time traces of the pinkish hues began to appear, and the clouded appearance of the lunar disc was very noticeable for some time past the beginning of totality. The time noted for the beginning of the total phase was 11^h 19^m 40^s. What impressed us in all the three hours of observing was the very changeful color of the light from the Moon during the first half of the period of totality. Some times the lunar surface would be more than half covered irregularly with a shading of a much deeper hue than that of the pinkish tint which prevailed elsewhere on the surface. It was also noticeable that the shading on the middle part of the disc was more persistent than in the region of the limb. At one time the shading seemed to change considerably in locality within a period of a few minutes.

After the middle of totality these perplexing shadows disappeared almost entirely and a nearly uniform pinkish hue covered all the central regions while the light near the limb was somewhat brighter than elsewhere on the surface. The seas would of course appear darker than the southern plateau regions, because they really are so and a uniform coloring should reveal this fact if the light were strong enough to show the contrast. The difference of color during the first and last parts of totality was so great as to be considerably surprising. During the last part, all the prominent features of the surface were very easily seen, and many of the lesser ones could be recognized.

At 12^h 42^m a light glow of the returning sunlight appeared on the limb of the Moon at the point where the shadow was begin-

ning to recede; at $12^h\ 48^m\ 20^s$ distinct sunlight is seen at that point. At $1^h\ 17^m$ the umbra leaves Tycho; at $1^h\ 28^m$ it passes the Tycho ray through Mare Serenitatis and its boundary is very nearly parallel with the ray. At $1^h\ 32^m$ the umbra leaves Mare Serenitatis, and the penumbra seems a little broader than before; at $1^h\ 45^m\ 30^s$ the umbra leaves Mare Crisium and at $1^h\ 51^m\ 20^s$ came the end of totality.

To us the most interesting part of these observations was the change of color in the pinkish tints that appeared during the period of totality and during a considerable part of each of the partial phases. The so-called copper-colored hue of which observers usually speak does not seem to us to be the best designation. To our vision the color was decidedly pinkish, showing a difference not easily distinguished by the ordinary definitions of the two terms for they are nearly the same in verbal expression.

Further than this, the shaded areas on the lunar surface, at times, claimed particular attention. During the first half of the period of totality this shading was remarkable. At one time two-thirds of the lunar surface from east to west was covered with an uneven darkening that shut out all the pinkish light from that region. The telescope did not reveal the slightest tinge of it as far as our eye could discern. But the north and south limbs were shining in pinkish light as at other times. We know that when the Moon is fully immersed in the shadow of the Earth, that it may be reached by refracted sunlight, and that the color of the sunlight falling on the lunar surface ought to be of a pinkish or of a copper-colored hue, because of the depth of atmosphere which the solar rays have traversed. Those rays must pass through a great depth to reach the observer from the sunward side of the Earth, they must pass through the same depth again on their way to the Moon, they must be reflected from the Moon and pass into the atmosphere for a second time and come finally to the observer's eye. Ordinary textbooks on astronomy explain how these solar rays get into the shadow of the Earth, and all readers will easily understand this when it is remembered that the effect of refraction is to bend the rays of light which pass within a few miles of the surface of the Earth downward through an angle of about $34'$, and as the same rays pass out of the atmosphere on the moonward side the same effect is produced a second time, so that a given ray of sunlight has suffered a total refraction of $1^\circ\ 8'$. But the greatest radius of the Earth's shadow is $1^\circ\ 2'$; so it is evident that these particular rays of sunlight just referred to must cross one another com-

ing around opposite sides of the Earth before they reach the Moon. Hence that sunlight which can get through so much atmosphere and is so refracted will fall on the Moon, and give the color seen, because other rays of light are absorbed on the way.

The ordinary explanation for the difference of tint or shading on the different parts of the lunar surface at the same time, is, that the rim of atmosphere surrounding the Earth through which the refracted light passes is clear or partly clouded so that differing amounts of sunlight will be transmitted. (See Young's *General Astronomy*, article 376). This explanation is sufficient for uniform coloring of the lunar disc, but is it clearly sufficient to explain the variation of color on different parts of the surface at the same time? If the entire rim of light in the atmosphere of the Earth, as seen from the Moon, is clear or wholly clouded it is plain enough to see that the Moon would be wholly obscured and no trace of it would be observed from the Earth in the one case, or, that it would be plainly visible with uniform color in the condition of a clear, terrestrial atmosphere. But if portions of the atmosphere were hazy or partly clouded should we see portions of the lunar surface unevenly illuminated? This view of the case is not easily accounted for by the explanation given above, because the refracted rays of sunlight from any portion of the terrestrial ring should spread over the entire disc of the Moon, unless the clouded portions in the Earth's atmosphere which effect the solar rays are very shallow, or cover very small areas north and south. Under such conditions it is possible that the explanation could hold good.

Brief notices of a few total lunar eclipses will be instructive respecting these interesting phenomena of color. Smyth's description of the lunar eclipse of Jan. 28, 1888, as given in Chamber's *Handbook of Astronomy*, page 331, says: "The phase of total eclipse began nominally at 10:30 G. M. T., but it was not until fully 20 minutes after this the last remains of the silvery shading along the west limb of the Moon had entirely disappeared. Up till the time that it did disappear, the familiar coppery hue, often seen in total eclipses of the Moon, was not at all uniformly spread over the Moon's disc. Indeed there was no more than a coppery patch somewhat to the east of the center of the disc for a long time, and I doubted whether this usual concomitant of a total lunar eclipse was going to be at all a conspicuous feature. However, as the time wore on, and the middle of the eclipse drew near, the whole disc (at 11:20) became overspread with the coppery hue. I speak of it under this name, because it is the term

usually employed, but in reality the tinge was more pink than coppery in the usual sense of that word, and it was much paler than usual; so much so indeed that in the middle of totality (11:30), it was easy enough not only to see the whole disc of the Moon, but also to identify some of the more conspicuous craters, such as Tycho, Copernicus and Kepler, as well as several of the larger seas." * * *

"Soon after 11:55, the atmosphere began to get hazy all around, and before the total phase ended (12:10) the pink hue had become greatly enfeebled, though it did not finally disappear for a considerable time—half an hour or more. The haze varied much from minute to minute, and every now and then, when a little denser, its effect on the Moon was to make it look like a perfect snowy sphere, and her globular form was brought out, with intense reality, constituting a sight of remarkable beauty."

The total lunar eclipse of May 19, 1848, was a remarkable one for the brightness of the lunar disc. Mr. Forester observed it at Bruges and described it as follows:

"I wish to call attention to the fact which I have clearly ascertained, that during the whole of the late eclipse of March 19, the shaded surface presented a luminosity quite unusual, probably about three times the intensity of the mean illumination of the eclipsed lunar disc. The light was of a deep red color. During the totality of the eclipse, the light and dark places on the Moon could be almost as well made out as on an ordinary dull moonlight night, and the deep red color when the sky was clearer, was very remarkable from the contrasted whiteness of the stars. My observations were made with different telescopes, but all presented the same appearance, and the remarkable luminosity struck every one.

The British Consul at Ghent, *who did not know there was an eclipse*, wrote me for an explanation of the 'blood red color of the Moon at 9 o'clock.' "

Another eclipse record gives a striking contrast from that last described. It was the total lunar eclipse of May 18, 1761. Observer Worgentin says that 11^m after the commencement of the phase, "the Moon's body had disappeared so completely that not the slightest trace of any portion of the lunar disc could be discerned, either with the naked eye or with the telescope although the sky was clear, and the stars in the vicinity of the Moon were distinctly visible in the telescope."

The explanation of the varied coloring of the lunar surface during the total phase of the lunar eclipse given in Chamber's *Hand-*

book of Astronomy, page 329, does not seem to us quite satisfactory. Speaking of the color of the light from the Moon, the author says: "On account of the variable meteorlogical condition of our atmosphere the quantity of light actually transmitted [through the terrestrial atmosphere] is liable to considerable fluctuations, and hence arises a corresponding variation in the appearances presented by the Moon's surface during her immersion in the Earth's shadow. If the portion of the atmosphere through which the solar rays have to pass is everywhere tolerably free from vapor, the red rays will be almost wholly absorbed, but not so the blue, and the illumination will be too feeble to render the Moon's surface visible. * * * If, on the other hand, the region of the atmosphere through which the solar rays pass be everywhere highly saturated, the red rays will be transmitted to the Moon in great abundance, and its surface will consequently be highly illuminated. * * * If, moreover, the region of the atmosphere through which the rays pass be saturated only in some parts and not in others, it follows that some portions of the Moon's disc will be invisible, while others will be more or less illuminated."

Mr. Chambers calls attention to the fact that Johnson does not consider that these explanations accord with the observed meteorological facts and that Monck holds the same view.

Mr. S. J. Corrigan of St. Paul has recently offered a new theory of which the following is a brief statement in his own words: "Another explanation which has not to my knowledge ever before been advanced is the following: It is a well known fact of physics that certain earthly substances after being exposed to direct sunlight, will, when taken into the dark, emit considerable light owing to the chemical action of the Sun's light vibrations upon the atoms of the substance, and quite recently many remarkable instances of radiative action from certain substances, even without previous exposure to sunlight have been discovered. Now, from time of "new Moon" to that of "full Moon," or about two weeks, the Moon's surface has been more or less exposed to the full intensity of the Sun's rays, which, since the Sun has no appreciable atmosphere, beat, continuously and unobstructedly, upon the matter of the lunar surface, so that it is not only possible, but also very probable, that, when the Sun's light is cut off from the Moon by the Earth's shadow, at time of total lunar eclipse, the Moon's surface matter will send out those faintly luminous radiations which constitute the "coppery hue" whereby the lunar disk is made faintly discernible while in total eclipse.

The two possible and very probable causes cited above co-operating would fully account for the phenomenon observed, and I have proposed this theory as a very probable one."

It seems to us that this whole matter is in considerable doubt and that it needs more investigation and discussion.

THE ORBIT OF β DELPHINI.

R. G. AITKEN.

This investigation was undertaken to determine whether the observations of a close and difficult double star are sufficiently accurate to make it worth while to correct the elements of its approximate orbit by differential methods. Such methods have been applied in researches on the orbits of some of the moderately easy binary and ternary systems, as, for example, in Seeliger's classic memoirs on Zeta Cancri. But the orbits of the more difficult binaries have generally been based in part or wholly upon the graphic construction of the apparent ellipse, though the resulting elements have occasionally been improved by the use of Klinkerfues's very convenient method of six position-angles.

The system of β Delphini seemed well adapted for such an investigation. Though discovered by Burnham as recently as 1873, it has already completed a revolution. The maximum distance does not reach 0″.7, and its minimum distance about 0″.2, does not fall below the resolving power of our larger telescopes. The pair has been, accordingly, well observed by all the leading double-star observers of the last quarter century, and a number of orbits have been computed. The two most recent ones, by See in 1895, and Burnham in 1898, are very similar in all their elements except the length of the semi-axis major.

The first step in the present investigation was to compare with these two orbits the observations of the star collected in Burnham's General Catalogue of his double stars. The comparison was made by drawing interpolating curves for the computed angles and distances derived from either orbit, and plotting the observed positions on the same scale. There seemed but little choice between the two curves for position-angles, See's representing the observed positions somewhat better at the two extremities, Burnham's those in the middle portion. But Burnham's distance-curve represents the distance-measures of the best observers far better than does See's.

Using See's elements as a basis, I next computed the following set of elements by Klinkerfues's method of six position-angles:

SYSTEM I.

P = 27.94 years	$a = 0''.475$
T = 1883.230	$\Omega = 179°.10$
$e = 0.363$	$i = 61\ .75$
	$\lambda = 352\ .80$

Interpolating curves were constructed from their elements, and the individual observations to 1902 compared with them, the measures having been taken from original sources. In this connection I wish to express my hearty thanks to Professor Schiaparelli, who kindly sent me the full details of his fine series of measures of this pair.

Angle measures that gave residuals exceeding 10° as the result of this comparison were rejected, unless the distance at the time of observation was as small as 0″.25. In this case the limit was set at 15°. Individual distance-measures were rejected only when they differed by 0″.2 from the computed, but an observer's mean result for any year was given weight zero if it differed by 0″.15 from the computed distances. The annual means were then formed by combining the measures of the various observers, assigning weights to each, depending in part upon the number of observations and in part upon the aperture of the telescope used.

When these annual means were compared with the elements given above, it appeared that the distances were satisfactorily represented, but the residuals in angle seemed to show systematic deviations. The measures of each observer had already been examined for systematic errors of observation, but only two series of observations—Schiaparelli's and Burnham's—were of sufficient extent to afford a basis for deriving reliable corrections for such errors, and their measures did not indicate appreciable errors of this kind.

A least-square solution therefore seemed unwarranted, and it was considered better to combine the annual means to form six normal position-angles and again apply Klinkerfues's method.

These normal angles, with dates and residuals, are as follows:

Date.	Mean Anomaly System I.	$\Delta\theta$ (O—C) System I.	Normal Angle	Weight.	$\Delta\theta$ (O—C) System II.
	°	°	°		°
1876.246	270	— 1.26	22.64	22	— 1.58
1882.066	345	+ 2.45	159.45	11	+ 1.49
1885.946	35	— 0.88	220.94	15	— 4.37
1888.662	70	+ 2.56	300.86	30	+ 1.18
18 .318	130	— 0.60	341.27	70	— 0.47
1899.526	510	+ 1.40	364.33	77	+ 0.93

The elements resulting from this new solution are:

SYSTEM II.

P = 27.66 years	$a = 0''.475$
T = 1883.10	$\Omega = 178°.90$
$e = 0.363$	$i = 60\ .90$
	$\lambda = 351\ .95$

The value of a was derived from several normal values of the observed distances based upon measures by Burnham, Barnard, Schiaparelli, Comstock, Hussey, and Aitken.

The last column in the table shows that these new elements represent all the normal places, except the first and third, better than System I. These two places—and the second as well—have much less weight than the last three; and it is to be remembered that they are based in large part upon measures with small telescopes. Considering this fact and the small apparent separation of the components of β Delphini in those years, we should expect larger errors in the measures made before 1887 than in the later ones.

I therefore regard System II as satisfactory, so far as the representation of the data at hand is concerned, and I think that the use of differential formulæ and a least-square solution will be of little service in improving these elements until more extended series of measures are available.

As the apparent orbital motion of this pair will increase rapidly during the next few years, I append an ephemeris, giving the relative positions of the two components according to Elements II for every tenth degree of mean anomaly to the date of the next periastron passage.

EPHEMERIS.

Date.	M.	θ	ρ
	°	°	″
1903.077	260	18.6	0.48
1903.845	270	23.6	0.43
1904.613	280	30.1	0.37
1905.382	290	38.9	0.31
1906.150	300	51.8	0.26
1906.918	310	71.2	0.21
1907.687	320	97.8	0.19
1908.455	330	125.7	0.20
1909.223	340	147.3	0.24
1909.992	350	162.8	0.28
1910.760	360	175.0	0.30

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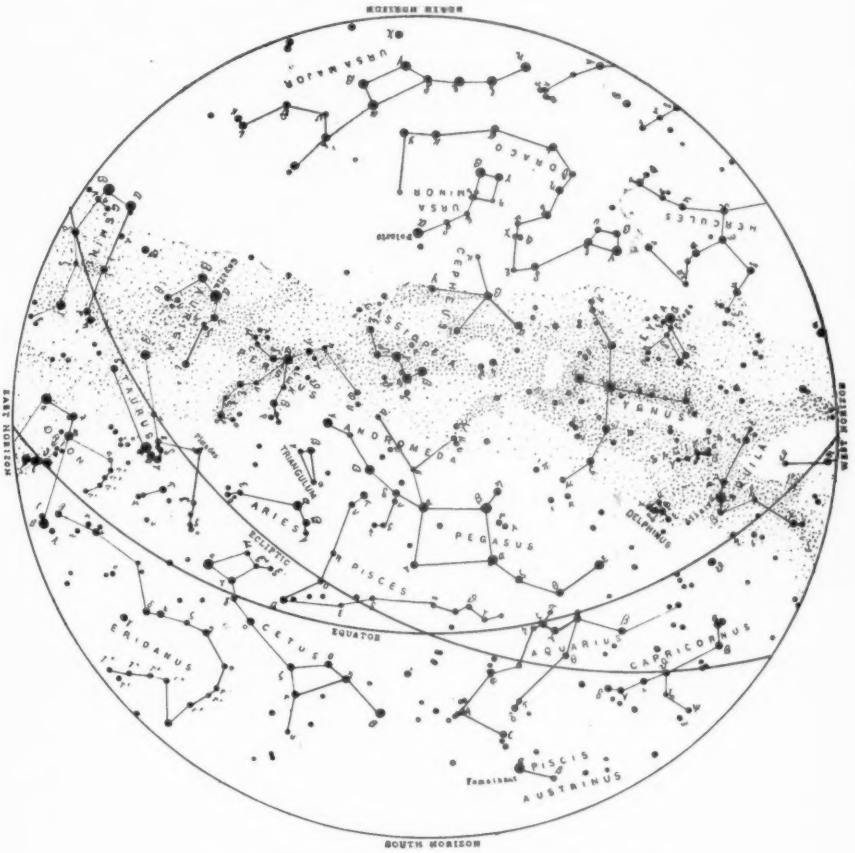
PLANET NOTES FOR NOVEMBER.

H. C. WILSON.

Mercury will be at greatest elongation, west from the Sun 18° 50′, on the morning of Nov. 4 and so will be visible as morning star, toward the southeast, during the first half of the month.



THE CONSTELLATIONS AT 9 P. M. NOVEMBER 1, 1902.

Venus will be at superior conjunction on Nov. 28 and so will be visible only during the first half of the month, and that so short a time before sunrise that it will be a difficult object.

Mars is in Leo, a little east of Regulus and will move slowly southeast during the month. Watchers for the Leonid meteors will therefore have to add this

planet in its proper place to the stars in the constellation from which the meteors radiate. The planet is easily recognized by its ruddy color and its brightness, which exceeds that of all the stars in Leo. The Moon will be in conjunction with Mars on Nov. 23, but will pass over 5° to the south of the planet.

Jupiter is at quadrature, 90° east from the Sun Nov. 1 and so must be observed early in the evening. Although the planet's altitude is so low, yet the astronomy class at Carleton College have obtained a number of good views of its surface markings during October, which gives encouragement to hope for more during November.

Saturn also may be observed early in the evening in the southwest, but at a lower altitude than Jupiter so that there is less hope of getting good views. The Moon will pass Saturn Nov. 6 and Jupiter on the evening of Nov. 7.

Uranus is too far to the west in the early evening to be observed.

Neptune will be at opposition next month and so will soon be observable during the whole of the night. It is 18′ south and about a half degree west of the star μ Geminorum on Nov. 1 and will move a little more than a half degree toward the west during the month, the declination of the planet remaining very nearly constant.

The Moon.

	Phases.		Rises. (Central Standard Time at Northfield Local Time 13m less.)		Sets.	
			h	m	h	m
Nov.	8	First Quarter.....	1	06 P. M.	12	05 A. M.
	14-15	Full Moon.....	5	05 "	8	05 "
	21-22	Last Quarter. 11		34 "	12	55 P. M.
	29	New Moon.....	6	51 A. M.	4	41 "

Occultations Visible at Washington.

Date. 1902.	Star's Name.	Magnitude.	IMMERSION. Washington M.T. h	m	Angle from N pt. °	EMERSION. Washington M. T. h	m	Angle from N pt. °	Duration. h	m
Nov. 15	B.A.C. 1272	6.3	16	04	89	17	07	265	1	03
16	*m* Tauri	5.1	13	51	50	14	52	308	1	01
18	λ Geminorum	3.6	15	57	117	17	13	270	1	16
21	16 Sextantis	6.9	16	35	180	17	09	229	0	34
24	*f* Virginis	5.9	14	07	108	15	06	293	0	59
25	*h* Virginis	5.5	18	22	64	19	12	348	0	50
26	λ Virginis	4.7	16	37	120	17	41	282	1	04

Sunspots.—The long dearth of sunspots was broken by the appearance between Oct. 3 and Oct. 6, of a quite conspicuous group, which on Oct. 6 consisted of 22 spots, one of them being about 34,000 miles in diameter with an umbra of about 8,000 miles diameter. This large spot was in advance, west, of all the others and on Oct. 6 had a curved left-hand spiral tail. On Oct. 7 the appearance had so changed that the tail was nearly all drawn into the penumbra, only a small spot remaining outside. The group of small spots following the leader numbered 20 Oct. 6, 18 Oct. 7, 15 Oct. 8, 13 Oct. 9, 6 Oct. 10, 0 Oct. 13, 0 Oct. 14, and 2 Oct. 15. Many of them were very small and all together would scarcely cover the area of the large spot. The following table gives the approximately measured position of the umbra of the large spot on several dates:

		C. S. T.	Longitude.	Latitude.	Area.
		h m	°	°	
Oct.	6	3 34	0.1	+ 9.4	0.000161
	7	2 04	359.5	+ 9.2	198
	8	2 05	359.0	+ 9.3	198
	9	3 17	358.6	+ 9.7	198
	10	3 09	358.1	+ 9.5	198
	13	2 30	358.1	+ 9.6	0.000127

The area is expressed in units of the visible hemisphere of the Sun. Another group of two spots was visible Oct. 7 and 8, the leading spot being in longitude 20°.6 and latitude + 21°.0.

On Oct. 18 the solar disk was entirely free from spots but on the 20th a new group was visible northwest of the center.

Phenomena of Jupiter's Satellites.

Central Standard Time.

	h	m					h	m			
Nov. 1	9	34	P. M.	II	Ec. Re.	Nov. 17	6	55	P. M.	II	Sh. In.
2	7	54	"	III	Tr. In.		7	13	"	II	Tr. Eg.
	9	00	"	I	Tr. In.		9	50	"	II	Sh. Eg.
	10	20	"	I	Sh. In.	18	7	23	"	I	Tr. In.
3	6	19	"	I	Oc. Dis.		8	41	"	I	Sh. In.
	9	57	"	I	Ec. Re.		9	43	"	I	Tr. Eg.
4	4	49	"	I	Sh. In.	19	4	09	"	II	Ec. Re.
	5	49	"	I	Tr. Eg.		4	43	"	I	Oc. Dis.
	7	09	"	I	Sh. Eg.		8	16	"	I	Ec. Re.
	8	44	"	IV	Ec. Dis.	20	4	13	"	I	Tr. Eg.
6	7	05	"	III	Ec. Re.		5	30	"	I	Sh. Eg.
8	6	36	"	II	Oc. Dis.		6	19	"	III	Oc. Dis.
10	4	33	"	II	Tr. Eg.		10	02	"	III	Oc. Re.
	7	14	"	II	Sh. Eg.	21	7	39	"	IV	Ec. Re.
	8	15	"	I	Oc. Dis.	24	5	08	"	III	Sh. Eg.
11	5	25	"	I	Tr. In.		6	57	"	II	Tr. In.
	6	45	"	I	Sh. In.	26	6	40	"	I	Oc. Dis.
	7	45	"	I	Tr. Eg.		6	48	"	II	Ec. Re.
	9	05	"	I	Sh. Eg.	27	5	06	"	I	Sh. In.
12	6	21	"	I	Ec. Re.		6	12	"	I	Tr. Eg.
	6	31	"	IV	Tr. In.		7	26	"	I	Sh. Eg.
13	5	52	"	III	Oc. Re.	28	4	40	"	I	Ec. Re.
	7	33	"	III	Ec. Dis.	29	6	37	"	IV	Tr. Eg.
17	4	16	"	II	Tr. In.						

NOTE.—In. denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse; Oc., occultation; Tr., transit of the satellite; Sh., transit of the shadow.

COMET AND ASTEROID NOTES.

Comet *b* 1902 (Perrine.)—This comet has swept across an immense arc of the sky during October and is now to be seen toward the southwest in the early evening. It is deviating somewhat from the ephemeris of Mr. Aitken given in our last number, p. 445, but not more than should be expected from a preliminary computation. On Oct. 14 the correction to the ephemeris was about + 1.6^m and + 19′. The comet is so bright that the observer has no difficulty in finding it with field-glasses and after it is once found it can just be distinguished with the naked eye. On Oct. 20 a short streak of the comet's tail could be seen with field-glasses, and with a 5-inch telescope the tail could be traced to a distance of about one and one-half degrees from the nucleus.

From the diagram of the comet's orbit which is given elsewhere, p. 479, it will be readily seen that after the first few days in November the comet will be hidden in the rays of the Sun, but will emerge on the other side in December. After that time it will continue for several months to be visible in the Southern Hemisphere.

Mr. F. E. Seagrave sends the following ephemeris, computed from Aitken's elements, given in the last number of POPULAR ASTRONOMY. This ephemeris, though considerably in error from the uncertainty of the elements, will indicate the general course of the comet.

EPHEMERIS OF COMET *b* 1902 (PERRINE).

Gr. Mdn. 1902.	α h	m	s	δ °	′	″	log *r*	log Δ	Brightness.
Nov. 17	16	34	1	−15	19	56	9.63707	0.11069	17.21
21	16	21	5	−17	10	47	9.60695	0.13095	18.01
25	16	7	16	−18	58	39	9.60551	0.14237	17.20
29	15	53	22	−20	44	13	9.63341	0.14503	14.94
Dec. 3	15	40	3	−22	28	28	9.68001	0.14026	12.32
7	15	27	25	−24	12	55	9.73351	0.12965	10.12
11	15	15	19	−26	0	14	9.78681	0.11453	8.49
15	15	3	13	−27	52	4	9.83683	0.09577	7.35
19	14	50	34	−29	50	38	9.88267	0.07393	6.58
23	14	36	53	−31	58	10	9.92435	0.04937	6.08
27	14	21	13	−34	15	17	9.96223	0.02253	5.78
31	14	2	37	−36	41	56	9.99679	9.99389	5.62
1903.									
Jan. 4	13	39	54	−39	15	20	0.02840	9.96420	5.57
8	13	11	30	−41	48	2	0.05749	9.93472	5.58
12	12	35	54	−44	4	41	0.08435	9.90723	5.60
16	11	52	18	−45	39	35	0.10929	9.88425	5.55
20	11	1	56	−45	59	40	0.13252	9.86877	5.35
24	10	9	6	−44	40	39	0.15426	9.86342	4.97

F. E. SEAGRAVE.

PROVIDENCE, R. I., Oct. 15, 1902.

Near Approach of Comet *b* 1902 to Mercury.—If Professor Aitken's elements of comet *b* (Perrine) on page 444 of POPULAR ASTRONOMY, October, 1902 are correct, there will be a near approach to Mercury on Nov. 29, this year, about 17^h Greenwich time. The following will show about the heliocentric positions of the two bodies:

	h		λ °	′	β °	′	″	log *r*
Nov. 29	17	Comet	225	18	−1	50	40	9.63581
29	17	Mercury	226	6	+0	14	0	9.65328

Distance between the two about 0.01771, or about 1,644,000 miles.

F. E. SEAGRAVE.

Comet *b*, 1902 (Perrine) has been observed here each evening since discovery. The nucleus shows well marked condensations, while the tail is quite wide and diffuse for a comet at such great distance from the Earth. It is growing brighter. EDGAR L. LARKIN.

LOWE OBSERVATORY, Echo Mountain, Calif.
September 9, 1902.

Comet *c*, 1902 (Griggs).—In *A. N.* 3816 is given an account by Mr. John Grigg of Thames, New Zealand, of his discovery of a comet on July 22 at 18^h 30^m Greenwich mean time. The position of the comet was then R. A. 11^h

35^m Decl. + 7° 0′ approximately. Mr. Grigg used a 3½ inch telescope by Wray and had no micrometer, or even cross-hairs in the eyepiece, by which to determine the position of the comet. Unfortunately the comet was not seen by other observers, although Mr. Grigg followed it up to Aug. 2, when the moonlight interfered, and gave notice to the New Zealand newspapers. From the best of his own rough observations Mr. Grigg computed the approximate elements:

$T = 1902$, June 20
$\omega = 292°\ 43'$ $\quad i = 18°\ 24'$
$\Omega = 217\ \ 48$ $\quad \log q = 9.7241$

From these very uncertain elements Mr. M. Ebell of Kiel has computed the following ephemeris:

1902.	R. A. h m	Decl. ° ′	log r.	log Δ.	Brightness.
May 20	4 13.7	+ 16 15	9.9407	0.2706	0.38
June 21	7 28.1	+ 16 27	9.7245	0.0937	2.29
July 23	11 28.5	+ 7 1	9.9567	0.0416	1.00
Aug. 24	14 21.2	− 2 54	0.1565	0.1896	0.20
Sept. 25	15 57.5	− 8 22	0.2855	0.3536	0.05
Oct. 27	17 04.0	− 10 39	0.3785	0.4773	0.02

As Mr. Griggs describes the comet as extremely faint at the time of discovery it will be seen from the column of computed brightness that there is little hope of its being found again.

Definitive Elements of the Orbit of Swift's Comet 1895 II.—In *Bulletin Astronomique* for Sept. 1902, Mr. M. Schulhof gives the details of a definitive determination of the elements of the comet 1895 II, discovered by Swift at Echo mountain, Aug. 20, 1895. Mr. Schulhof combined all the published observations, some four hundred in all, extending from Aug. 21, 1895 to Feb. 5, 1896, into seven normal places, and was able to represent them all within 1″.5. The elements for 1902, corrected for perturbations by Jupiter and Saturn are:

Epoch 1902 Oct. 1.0 Paris M. T.

$M = 354\ \ 53\ \ 43.3$ $\quad \phi = 40\ \ 36\ \ 57.7$
$\pi = 338\ \ 15\ \ 49.0$ $\quad \mu = 490''.903$
$\Omega = 170\ \ 16\ \ 56.0$ $\quad \log a = 0.572674$
$i = \ \ 3\ \ 00\ \ 42.7$

An ephemeris computed from these elements was given in the September No. of Popular Astronomy, and a continuation for the first part of November is here given.

At the present writing the comet has not been refound. Mr. Schulhof says that "at the opposition of 1902 the comet will remain very faint; there may be hope nevertheless that it may be found with the aid of some of the more powerful instruments. There will be no possibility of observing it in 1910 and 1917. In 1924 its luminous intensity will be almost the same as in 1902, but the limits of uncertainty of its position will then be four times as great."

Search Ephemeris for Swift's Comet 1895 II.

[Continued from page 380.]

Paris Mdn.	R. A. h m s	Decl. ° ′	log Δ	1: $r^2\Delta^2$
Oct. 31	19 13 38	− 21 24.5	0.1030	0.364
Nov. 2	21 10	21 16.1		
4	28 46	21 06.1	0.1057	
6	36 25	20 54.7		
8	44 06	20 41.8	0.1089	0.356
10	51 49	20 27.4		
12	19 59 33	− 20 11.6	0.1125	

New Asteroids.—The following have been added to the list of new asteroids since our last note:

					Local M. T.	R. A.	Decl.	Mag.
					h m	h m	° ′	
1902	JO	Wolf	Heidelberg	Sept. 3	12 55.8	23 54.7	−0 13	13
	JP	"	"	3	12 55.8	23 56.7	+1 14	12
	JQ	"	"	3	12 55.8	0 13.4	+1 20	12.5
	JR	"	"	3	12 55.8	0 09.2	−1 21	12
	JS	"	"	7	10 38.9	0 10.3	−0 13	13
	JT	Carnera	"	26	11 07.4	0 53.7	+1 43	12.7

JN has been found to be identical with (311) Claudia.

Elements of Planetoid (416) Vaticana.—In *A. N.* 3816 Mr. G. Boccardi gives elements of this planet, depending upon 7 normal places obtained from the observations in 1886:

EPOCH 1902, OCTOBER 21.5 BERLIN M. T.

$M = 114°\ 14'\ 16''.42$
$\omega = 195\ 24\ 58\ .08$ } 1900.0 $\phi = 12°\ 35'\ 49''.55$
$\Omega = 58\ 30\ 32\ .57$ } 1900.0 $\mu = 761''.66109$
$i = 12\ 55\ 43\ .44$ } 1900.0 $\log a = 0.4454966$

An ephemeris is given for October and part of November 1902. The portion for November follows:

Berlin Midnight.	R. A.	Decl.	log *r*.	log Δ
	h m s	° ′ ″		
Nov. 1	1 53 04.49	+2 08 00.0		
2	52 11.63	2 06 56.1		
3	51 19.31	2 05 58.3	0.49903	0.34078
4	50 27.56	2 05 07.0		
5	49 36.44	2 04 22.0		
6	48 45.99	2 03 43.8		
7	47 56.25	2 03 12.1	0.49989	0.34536
8	47 07.26	2 02 47.3		
9	46 19.06	2 02 29.3		
10	45 31.70	2 02 18.3		
11	1 44 45.19	+2 02 14.3	0.50075	0.35076

Magnitude about 12.1.

VARIABLE STARS.

Variable Stars in a Nebula.—In the Publications of the *Astronomical Society of the Pacific* No. 86, Mr. C. D. Perrine announces the discovery of two variable stars in the nebula N. G. C. 7023. Their positions from the central star are

	Position Angle.	Distance.
	°	″
A	6.2	107.1
B	138.6	87.4

The discovery was made by the comparison of photographs taken Nov. 7, 1901 and Aug. 27, 1902. Another photograph taken Sept. 1, 1902, indicates slight but certain changes so that the periods of the two variables are probably short. The magnitudes assigned on the several dates were:

		A	B
1901	Nov. 7	14½	16¾
1902	Aug. 27	16¼	15¾
	Sept. 1	16½	15½

Mr. Perrine says: "N. G. C. 7023 is an especially interesting nebula from its suggestive location in a region comparatively devoid of stars. The nebula is about 15′ across, and consists of irregular streamers and masses of nebulosity folded about a 7th magnitude star."

Minima of Variable Stars of the Algol Type.

[Greenwich Mean Time beginning with midnight. The hours greater than 12 are those of the afternoon. To obtain Eastern Standard time subtract 5 hours; for Central Standard subtract 6 hours, etc.]

U Cephei.

	d	h
Nov.	3	9
	5	21
	8	9
	10	20
	13	8
	15	20
	18	8
	20	20
	23	8
	25	19
	28	7
	30	19

Algol.

Nov.	3	1
	5	22
	8	19
	11	16
	14	12
	17	9
	20	6
	23	3
	26	0
	28	20

λ Tauri.

Nov.	2	4
	6	3
	10	2
	14	1
	17	23
	21	22
	25	21
	29	20

R Canis Majoris

Nov.	2	1
	3	5
	4	8
	5	11
	6	14
	7	18
	8	21
	10	0
	11	4
	12	7
	13	10
	14	13
	15	17
	16	20
	17	23

RCan. Maj. Con.

	d	h
Oct.	19	2
	20	5
	21	9
	22	12
	23	15
	24	19
	25	22
	27	1
	28	4
	29	8
	30	11

S Cancri.

Nov.	8	16
	18	3
	27	15

S Antliæ.

Period $7^h\ 46^m.8$

Nov.	1	18
	2	18
	3	17
	4	16
	5	16
	6	15
	7	14
	8	14
	9	13
	10	12
	11	12
	12	11
	13	10
	14	10
	15	9
	16	8
	17	8
	18	7
	19	6
	20	6
	21	5
	22	4

S Antliæ Con.

	d	h
	23	4
	24	3
	25	2
	26	2
	27	1
	28	0
	29	0
	29	23
	30	23

U Coronæ.

Nov.	3	21
	7	8
	10	19
	14	6
	17	16
	21	3
	24	14
	28	1

U Ophiuchi.

Nov.	1	14
	2	10
	3	6
	4	2
	4	22
	5	18
	6	14
	7	10
	8	7
	9	3
	9	23
	10	19
	11	15
	12	11
	13	7
	14	3
	15	0
	15	20
	16	16
	17	12
	18	8
	19	4
	20	0

RX Herculis.

	d	h
Nov.	1	16
	2	13
	3	10
	4	8
	5	5
	6	2
	7	0
	7	22
	8	19
	9	16
	10	14
	11	11
	12	8
	13	6
	14	3
	15	0
	15	22
	16	19
	17	16
	18	14
	19	11
	20	8
	21	6
	22	3
	23	0
	23	22
	24	19
	25	16
	26	14
	27	11
	28	8
	29	6
	30	3

Z Herculis.

Nov.	1	10
	3	11
	5	10
	7	11
	9	10
	11	11
	13	10
	15	10
	17	9
	19	10
	21	9
	23	10
	25	9
	27	10
	29	9

SY (V^3) Cygni.

	d	h
Nov.	2	13
	8	13
	14	13
	20	13
	26	14

SW (U^3) Cygni.

Nov.	4	19
	9	9
	13	23
	18	13
	23	2
	27	16

W Delphini.

Nov.	5	14
	10	10
	15	5
	20	1
	24	20
	29	15

Y Cygni.

Nov.	2	6
	3	22
	5	6
	6	22
	8	6
	9	22
	11	6
	12	22
	14	6
	15	22
	17	6
	18	22
	20	6
	21	22
	23	6
	24	22
	26	6
	27	22
	29	6

Variable Stars of Short Period not of the Algol Type.

	Minimum.	h	Maximum.	h		Minimum.	h	Maximum.	h
T Monocerotis	Nov. 1	3	Nov. 9	1	δ Cephei	Nov. 14	16	Nov. 16	7
X Sagittarii	1	7	4	4	W Sagittarii	15	4	18	4
β Lyræ	1	10	4	19	X Sagittarii	15	8	18	5
S Sagittæ	1	23	5	9	T Vulpeculæ	17	12	18	21
Y Sagittarii	2	15	4	10	U Sagittarii	18	14	21	13
δ Cephei	3	23	5	14	S Sagittæ	18	17	22	3
T Vulpeculæ	4	5	5	14	Y Sagittarii	19	22	21	17
ζ Geminorum	4	6	9	6	δ Cephei	20	1	21	16
W Geminorum	5	0	7	15	η Aquilæ	20	4	22	10
U Sagittarii	5	2	8	1	W Geminorum	20	21	23	12
η Aquilæ	5	19	8	1	β Lyræ	20	19	23	21
W Sagittarii	7	13	10	13	U Aquilæ	21	16	23	20
U Aquilæ	7	15	9	19	T Vulpeculæ	21	23	23	8
β Lyræ	7	21	10	23	X Sagittarii	22	8	25	5
X Sagittarii	8	7	11	4	W Sagittarii	22	18	25	18
Y Sagittarii	8	9	10	4	ζ Geminorum	24	13	29	13
T Vulpeculæ	8	15	10	0	δ Cephei	25	10	27	1
δ Cephei	9	8	10	23	Y Sagittarii	25	17	27	12
S Sagittae	10	8	13	18	T Vulpeculae	26	9	27	18
U Sagittarii	11	20	14	19	S Sagittæ	27	3	30	13
X Cygni	13	0	16	19	β Lyræ	27	6	30	15
η Aquilæ	13	0	15	6	η Aquilae	27	8	29	14
T Vulpeculæ	13	2	14	11	T Monocerotis	28	3	6	1
W Geminorum	13	3	15	18	W Geminorum	28	14	1	5
β Lyrae	13	8	17	17	U Aquilæ	28	16	30	20
Y Sagittarii	14	4	15	23	X Sagittarii	29	8	2	5
ζ Geminorum	14	9	19	9	X Cygni	29	9	6	4
W Virginis	14	10	22	14	δ Cephei	30	19	2	10
U Aquilæ	14	15	16	19	T Vulpeculæ	30	20	2	5

Maxima and Minima of Long Period Variables.

[Computed from Chandler's "Third Catalogue" by Misses Ida I. Watson and Helen M. Swartz of Vassar College Observatory.]

Maxima.			Maxima.		
Date Dec	No.	Star.	Date Dec.	No.	Star.
1	103	T Andromedae	24	8290	R Pegasi
1	107	T Cassiopeae	25:	419	U Andromedae
2	8591	V Cephei	30	5770	R Herculis
4	6512	T Herculis	31	5776	X Scorpii
5	782	R Arietis	Minima.		
7	3825	R Ursae Majoris	6:	294	W Cassiopeae
7	5338	U Bootis	10	3186	T Cancri
7	6682	X Ophiuchi	14	5889	U Herculis
9:	6871	V Lyrae	17	5583	X Librae
11	5931	S Ophiuchi	17	7431	S Delphini
13	4521	R Virginis	18	466	U Piscium
16	4492	Y Virginis	22	715	S Arietis*
16:	6894	S Lyrae	23	2100	U Orionis
23	1717	V Tauri	26	6905	R Sagittarii
23:	7260	Z Aquilae	30	7252	W Capricorni
23	7659	T Capricorni	31	806	o Ceti

Maxima of U Pegasi.

Period $4^h\ 29^m.8$. The minimum occurs $2^h\ 15^m$ after the maximum.

	d	h		d	h		d	h		d	h
Nov.	1	3	Nov.	9	4	Nov.	17	1	Nov.	25	3
	2	1		10	3		18	4		26	1
	3	4		11	1		19	3		27	4
	4	3		12	4		20	1		28	3
	5	1		13	3		21	4		29	1
	6	4		14	1		22	3		30	4
	7	3		15	4		23	1			
	8	1		16	3		24	4			

N.

PHOTOGRAPH OF A LEONID METEOR TRAIL

TAKEN WITH THE 6 INCH STAR CAMERA AT GOODSELL OBSERVATORY, NOVEMBER 14, 1901.

GENERAL NOTES.

The Leonid Meteors.—The Leonids are due on the mornings of Nov. 15 and 16 but the Moon will be full and so will diminish the effect of whatever display there may be. If cameras are used, it will be necessary to change the plates at least every half hour, to prevent excessive fogging from the moonlight. We will furnish copies of our chart of the radiant to any who may desire to use them.

Photograph of a Leonid Meteor Trail.—The meteor trail, which is reproduced in Plate XVI, was photographed at Goodsell Observatory on the night of Nov. 14. 1901 with the 6-inch Brashear photographic doublet. The field photographed on the original plate was about 15° in diameter, with its center on the star Heis 44, which is about 1° west of the radiant. Although the plate was exposed from $14^h\ 20^m$ to $18^h\ 00^m$ C. S. T. only one meteor was bright enough, within the field, to produce an impression upon the plate. This dropped nearly south from the radiant expiring at a point almost midway between η and γ Leonis. The trail photographed is less than 1° long, very faint at first, gradually brightening and broadening then narrowing, with at the end a more sudden increase and decrease forming an arrow head. The cut is enlarged 6 diameters from a portion of the original photograph and the fine details of the structure of the trail have been mostly lost in the reproduction. In the original the middle line only of the trail is dense, bordered on both sides by a fuzzy or feathery penumbra with slightly recurved filaments. The brightest stars shown in the cut are of about the eighth magnitude.

New Astronomical Observatory for Wesleyan University.—There will soon be erected a new astronomical Observatory for Wesleyan University at Middletown, Conn., at a cost of $40,000. The money is given by a brother of Professor J. M. Van Vleck of the department of mathematics and astronomy and vice president of the University.

Triangulation of the Hyades Group.—*Astronomische Nachrichten*, No. 3818-19 contains an important article on the triangulation of the Hyades group of stars by Von Carl W. Wirtz. It gives an extended statement of the work done, the instrument used and full information in detail. A cut accompanies the paper showing the positions of the stars of the group, their magnitudes, and the lines of triangulation chosen for the survey. There is also a catalogue of 69 stars belonging to the group whose right ascensions and declinations have been determined for the epoch of 1900.0. The heliometer was the instrument used.

Bright Star-like Point on the Moon.—The night of October 10 was perfectly clear in Cambridge, and occasion was taken to look at the star-like point announced by Mr. Jones as visible in Lambert. No such point was found, but it was seen that Γ near it was very brilliant and striking, on account of its dark surroundings. A careful comparison however showed that it was no more brilliant than are other snow peaks under similar circumstances. It was concluded that Mr. Jones' identification was at fault. His observation that the point showed a diffraction ring however was new to me, and should if possible be confirmed.

WILLIAM H. PICKERING.

"Astronomy without a Telescope," is the title of a work to be issued in October from *Knowledge* office. The volume is written by E. Walter Maunder F. R. A. S., in a popular style, and is fully illustrated.

The House of Maria Mitchell.—*Science* informs us that "an association has been formed to buy the house in Nantucket in which Maria Mitchell, the astronomer, was born. It is proposed to place there her library and to establish a museum."

The July Meteors.—On the night of the 30th quite a number of Aquarids were seen.

Date.	Hour E. S. T.	Aquarids.	Date.	Hour E. S. T.	Aquarids.
July 30	8.00—12.00	12	July 30	2.00—2.15	6
	12.00—12.15	5		2.15—3.00	2
	12.15—12.30	4		2:30—2.45	6
	12.30—12.45	3	" 31	9.35	1
	12.45— 1.00	5	Aug. 7		1
	1.00— 1.15	5	8		1
	1.15— 1.30	3	9		6
	1.30— 1.45	3	12		9
	1.45— 2.00	8			—
		Total.....			..80

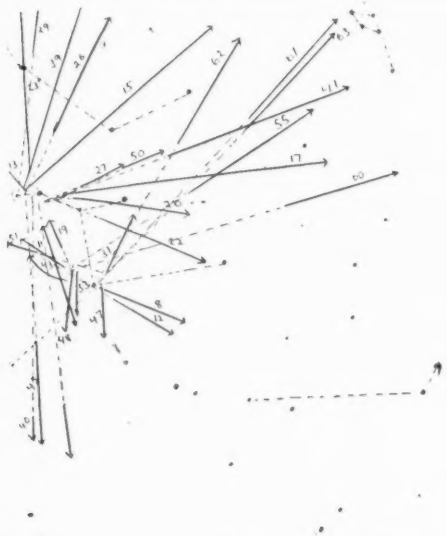


CHART OF AQUARID METEORS, JULY 1902.

The meteors were mostly slow and faint with long paths. Five out of 68 meteors left trails. Regular count was kept between 12:00 and 2:45. Average 1 in every 3⅓ minutes.

ROBERT M. DOLE.

OGUNQUIT, Me.

The August Perseids.—The Perseids were very active this year. The maximum probably came on August 12 at 14.30 hours.

Date.	Hour.	Perseids.	Date.	Hour.	Perseids.
July 28		1	Aug. 12	2.30— 2.45	17
30	10.00— 3.02	37		2.45— 3.00	17
31		3		3.00— 3.21	16
Aug. 3		2	13	11.40—11.45	7
4		1		11.45—12.00	5
7		8		12.00—12.15	9
8		11		12.15—12.30	11
9		14		12.30—12.45	9
12	7.30—11.00	37		12.45— 1.00	12
12	11.20—11.30	6		1.00— 1.15	9
	11.30—11.45	13		1.15— 1.30	10
	11.45—12.00	8		1.30— 1.45	11
	12.00—12.15	15		1.45— 2.00	17
	12.15—12.30	20		2.00— 2.15	13
	12.30—12.45	17		2.15— 2.30	16
	12.45— 1.00	16		2.30— 2.45	12
	1.00— 1.15	19		2.45— 3.00	17
	1.15— 1.45	42		3.00— 3.18	18
	1.45— 2.00	11			
	2.00— 2.15	26	Total.....		561
	2.15— 2.30	18			

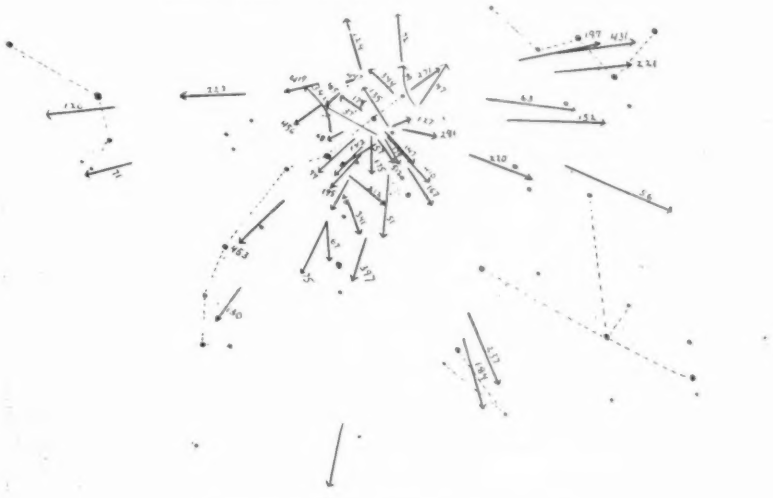


CHART OF PERSEID METEORS, August 1902.

Average on Aug. 12 between 11.20 and 3.21 was about 1 in a minute and a half. Average on Aug. 13 between 11.40 and 3.18 was about 1 in a minute and a third. Several times on the 12th and 13th there were several a minute.

On Aug. 12 at 12.53 there were 4
" " " " 1.04 " " 4
" " " " 1.34 " " 4
" " " " 2.04 " " 4
" " " " 2.27 " " 4
" " " " 1.59 " " 4

The number of shooting stars was quite large. On Aug. 12, 55 shooting stars were seen and 41 on Aug. 13. Many individual meteors were very bright.

On Aug. 12 at 11.27 one shot which was — 2
" " " " 11.31 " " " " — 2
" " " " 11.34 " " " " — 3
" " " " 11.35 " " " " — 2
" " " " 12.01 " " " " — 2
" " " " 12.26 " " " " — 6
" " " " 12.38 " " " " — 3
" " " " 12.42 " " " " — 2
" " " " 12.42 " " " " — 2
" " " " 12.44 " " " " — 2
" " " " 12.57 " " " " — 2
" " " " 1.55 " " " " — 2
" " " " 2.12 " " " " — 2
" " " " 2.18 " " " " — 3
" " " " 3.06 " " " " — 2
" " 13 " 12.13 " " " " — 2
" " " " 12.47 " " " " — 2
" " " " 1.53 " " " " — 3

148 out of about 500 meteors left trails.

NUMBER BY MAGNITUDE.

Magn.	— 6	— 5	— 4	— 3	— 2	— 1	0	1	2	3	4	5	6
No.	1	1	1	9	23	30	56	45	64	117	78	30	12

The meteors were usually white or yellowish white leaving yellow or green trails.

No.	Date.	Hour E. S. T.	Color.	Remarks.
37	July 30	3.02	4 W	left a 4 R trail
44	Aug. 7	10.02	4 W	left a 4 R trail
46		10.32	3 W	
47		10.32	3 W	
49		10.44	3 W	
51	7	10.54	4 W	
56	8	11.56	1 W	left a 4 W trail
63	9	9.10	4 W	
67		11 07	2 W	left a 3 R trail
68		11.15	2 W	left a 3 R trail
71		11.55	1 W	left a 4 W trail
72		11.58	1 W	left a 4 W trail
75	9	12.07	3 W	left a 5 W trail
104	12			
124		11.37	4 W	
126		11.39	— 1 W	
127		11.40	3 B	
130		11.42	3 W	
135		11.49	4 B	
143		12.02	4 W	
147		12.06	5 B	
152		12.12	2 W	left a 4 yel. trail
167		12.22	4 W	
175		12.29	4 W	
178		12.31	1 W	left a 3 W trail
179		12.35	5 W	
195		12.46	3 W	
197		12.49	3 W	left a 5 W trail
212		1.03	5 W	
219		1.09	4 W	left a 6 W trail
220		1.09	4 W	" " " "
221		1.10	2 W	left a 4 W trail
2..3		1.12	3 W	left a 5 W trail
237		1.22	0 W	left a 4 W trail

No.	Date.	Hour E. S. T.	Color.	Remarks.
271		1.54	5 W	
281		2.02	5 B	
341		2.43	4 W	left a 5 W trail
344	12	2.50	5 W	
397	13	12.16	4 B	
410		12.37	4 W	
415		12.41	4 W	left a 5 W trail
419		12.48	4 R	left a 3 W trail
431		1.08	1 W	left a 4 W trail
447		1.31	2 W	left a 3 W trail
453		1.40	5 B	
456	13	1.44	1 W	left a 4 W trail

The meteors near the radiant were usually faint and left trails, the ones in the northwest and southeast were usually bright and without trails, and the ones overhead in Cygnus very long and bright leaving broad trails. The shower this year was very good and some very bright ones were observed.

ROBERT M. DOLE.

OGUNQUIT.

The Absurdities of the Almanac.—A brief article dealing with the so-called absurdities of the almanac first appeared in the *Academy*, then in *The Living Age*, No. 3041, page 188. The writer, F. Legge, notices some sweeping changes advocated by a distinguished French astronomer, and concludes that although the changes suggested may be desirable, it will probably be a very long time before they will be adopted.

Some of those are to have the civil year begin on March 21, the first day of the year should be Monday and the last Sunday, that the "ridiculous" names of the months should be changed to more worthy ones, "representing the qualities, or at least the intellectual tendencies of humanity," such as, Truth, Science, Wisdom, Justice, Honor, Kindness, Love, Beauty, Humanity, Happiness, Progress and Immortality. The year would be divided into quarters, as now, the first month of which should have 31 days, and the remaining two 30 days each. This arrangement would make the year always begin on Monday, for example, if desired, and then always end on Sunday. The ample provision for holidays to make the year, month and day work well together, for a long time, by this new plan, is suggestive and ingenious.

Hypothesis Regarding the Nature of Solar Prominences.—Professor W. H. Julius has described before the Royal Academy of Sciences (Amsterdam) and published in its *Proceedings*, a theory as to the nature of solar prominences.

It may be remembered that Professor Julius accounted for the doubling of the arcs in the spectrograms obtained by him during the last total solar eclipse, by saying that it was due to the anomalous dispersion of the chromospheric light, and he now applies this theory of anomalous dispersion to account for solar prominences. He abandons the idea of the existence of various layers of different materials in the solar atmosphere, and suggests that "throughout the gaseous body, as well inside as outside the critical sphere, the various elements are altogether intrinsically mixed (granting that in the mixture the quantity of materials with greater specific gravity must grow with the depth)." It is suggested that, in the whirls formed by the ascent and descent of heated gases combined with the rotational velocity of the solar atmosphere, we get anomalous dispersion at the points where two or more of these whirls intersect and break each other;

and the author goes on to propose "that the whole chromosphere with all its prominences is nothing but this system of waves and whirls, made visible within shorter or longer distances from the Sun's edge by anomalous dispersion of light, coming from deeper layers."

Professor Julius also points out that this theory abolishes the necessity for supposing the immense velocities which Fenyi and others have observed in connection with solar prominences, because it suggests that there is not a transmission of material, but only successive appearances of the same phenomena at various heights. He likens this to the apparent velocity of the line of foam caused by water waves breaking on a coast which is inclined to their wave-fronts.—*Nature*, Sept. 4, 1902.

Photographic Atlas of the Moon.—In a communication to *La Société Française de Photographie*, Monsieur Wallon gives an account of the method adopted by Messrs. Loewy and Puiseux, of the Paris Observatory, in making their atlas of the Moon. This is a work upon which they have been engaged for some years, and it will be of great value to astronomers. The plates are being reproduced on an enlarged scale by photogravure from negatives taken with the large equatorial, which is the most powerful instrument of the Paris Observatory. The photographic objective of the great equatorial was made by Messrs. Henry, and is of 60 cm. aperture and 18 metres focus. Usually the lens is stopped down to 54 cm. aperture. The correction of the lens is for the chemical rays, and the adjustment of the focus is repeated each month photographically by trial and error. The image of the Moon in the original negatives varies between 160 and 180 mm., according to the distance of the satellite from the Earth. The exposures with Lumière, or Helios, extra rapid plates vary between ½ and 4 sec., according to the phase, and the clearness of the atmosphere. The lens remains stationary, but the plate is moved by a Foucault clock and cylindrical gearing. As the movement of the Moon is variable, the problem is a complicated one, but 3,600 different rates of speed may be obtained by the mechanism, and it is therefore easy to make suitable adjustments. The shutter is a sheet of aluminium placed immediately in front of the plate. It slides in grooves, and is controlled by hand, by means of a rod and crank, so that the brightest parts of the Moon's surface receive least exposure. A tubular sky-shade 1.60 m. in length is placed on the front of the lens when photographs are made during twilight. The plates are developed with amidol and anhydrous sulphite of soda to preserve the greatest transparency in the deposite. The enlargements are made by electric arc light and the lens used is a Lerebours and Secrétan single achromatic meniscus of 310 mm. focus, with convex surface to the light, stopped down to 10 mm. This gives perfect definition of the grain of the negative over the entire surface of the enlargement 80 c. by 80 c. Various other lenses have been tried, but none have given such satisfactory results. Different methods of focussing have been tried, but the best criterion of definition has been found in the grain of the plate as seen in the image upon a white screen. A slight displacement of the lens removes the grain and gives a more agreeable visual image, but for exactness the grain is retained. To obtain equality of exposure and full detail in the shadows, the light is passed through a screen with a slit a few millimetres wide before it reaches the condenser. This screen is kept in motion, and can be used locally. The scale of enlargement varies between 10 and 16 times. The plates are backed with dextrine and red ochre. About 400 photographs have already been made, but a few difficult phases of the Moon are still wanting to complete the atlas.—(*Brit. Jour. Phot.*, Sept. 26, 1902.)

The Lunar Eclipse.—I observed the eclipse this morning under favorable conditions, the sky clearing just before the commencement of totality. I observed a strange phenomenon which I have never before seen at any total eclipse, just before and during totality. With an opera glass I saw a dark belt as per diagram extending three-fourths across the disk, which at first I ascribed to a cloud, but when after several minutes no motion could be detected, I attributed it to defective vision. At the middle of the total phase, I gave the glass to Mr. W. A. Brink, not telling him what I had seen. This morning I asked him to make me a drawing of what he saw, and to my astonishment he drew it just as I had seen it, with the difference that his young eyes traced it clear across the disk.

The opera glass is first class, of the Zeiss pattern, made by the celebrated makers of optical goods, Warner & Swasey, of Cleveland, Ohio, and I can hardly believe the delusion if it was one, was therein.

The belt was quite dark—in strong contrast with the bright patches each side, which shone with a luster as white and bright as Venus. No color was seen by either of us, though intently looked for. The south patch of luminosity was much brighter than the north one. Both of us observed the disk with each eye, and with each lens, and with both and in every case the dark belt was seen.

From the eastern limb, the belt was of equal width for three-fourths its length, then narrowed to a blunt point, as represented. If the belt was seen by

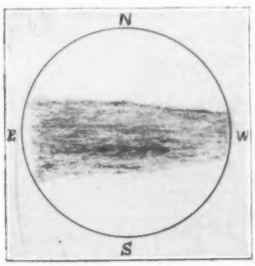


Fig. 1.—Dark Shadow as drawn by W. A. Brink.

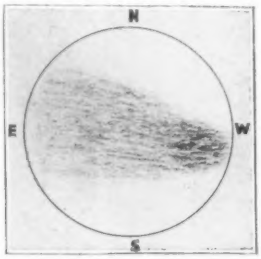


Fig. 2.—Dark Shadow as drawn by H. C. Wilson.

no one else, it looks as if I must ascribe its appearance to a defect in the opera glass, which at present I cannot believe. LEWIS SWIFT.

MARATHON, N. Y., Oct. 17, 1902.

P. S.—Since the foregoing was written, I see by the daily papers that Dr. Brooks also saw the dark belt across the Moon's disk, which relieves me of much anxiety as to the reality of what I saw—thought it might be an optical illusion. Mr. Brink tells me he saw the dark belt with his naked eye. Whatever it was, I am sure that nothing like it has ever been seen before. The belt was black not brown.

The Lunar Eclipse.—The shadow referred to by Professor Swift in the above note was observed by the writer at Goodsell Observatory. A pair of field glasses of 1½ inches diameter was used, and the change in character of the shadow as the eclipse progressed was very marked. The west side of the Earth's shadow which the Moon entered first was very dark. No color could be seen upon the immersed part of the Moon's disk until $10^h\ 35^m$ C. S. T., when the

southern and northern parts began to show a slight trace of red. At the beginning of totality the north and south portions of the disk were of a pinkish red color but across the equator was a dark shadow in which no detail could be seen at all. This was at first attributed to the effect of flying clouds and to the fact that the shadow covered the naturally dark portions of the Moon's surface, but it did not follow the outlines of those portions, and after about half an hour brightened up so that it wat not at all noticeable. At $11^h\ 46^m$ the whole disk was of a ruddy hue except a small spot near the west limb. At $12^h\ 33^m$ the N. E. limb was quite bright while the S. W. was dark, but the ruddy color could be seen in all parts; the center was not especially dark. At no time during totality did any part of the disk approach whiteness in color until a few minutes before the end, when that portion which was about to emerge was nearly white. As the shadow passed off it was noticeable that the ruddy color extended close up to the bright part of the Moon's disk, there being no such black edge as was seen when the eclipse began. The sketch shown in Fig. 2 was made at $11^h\ 25^m$ C. S. T.

H. C. WILSON.

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